Essays on the Effects of Political Institutions on Development Policies

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ABSTRACT

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This dissertation examines the relationship between political institutions and development policies across a wide array of policy arenas. It consists of three essays. In the first essay, I examine how corruption in political institutions affects citizens' attitudes towards proposed policy reforms that should yield long-run benefits. I argue that where corruption in political institutions reduces citizens' benefits from existing programs, governmental promises to deliver benefits via reforms are less credible. Thus, citizens will cling to inefficient policies not because they are unable to recognize the benefits of reform but because they do not trust political institutions to implement reforms in ways that will benefit them in practice. I use this logic to explain why citizens frequently resist attempts to reform the economically and environmentally costly practice of setting domestic gasoline prices below market prices. To reveal these patterns, I rely on original survey and administrative data from Indonesia. The second essay maintains the focuses on the quality of political institutions and natural resource governance but from a more macro perspective. In this essay, I argue that political regimes and political time horizons shape financial arrangements between governments and multinational oil companies. This essentially asks the reverse of a central question in comparative politics. Rather than asking how oil income affects political institutions, I ask how political institutions motivate politicians to make policy choices that increase or decrease the government's access to oil income over time. To do so, I utilize an original dataset on financial arrangements between host countries and multinational oil companies, as reflected in historically confidential oil contracts. The final essay travels to a different substantive area of development policy, yet allows for a critical role for political institutions. This essay argues that the relationship between developing country governments and foreign aid donors should be conditional on

the quality of political institutions, with aid donors giving countries with institutions better able to commit to selecting policies that promote development wider latitude to direct foreign aid resources towards local priorities. Instead, I find that political and security alliances shape whether donors give developing country governments more "ownership" over aid flows. Overall, the dissertation deepens understanding of the relationship between the quality of political institutions and policies within developing countries, while offering insights into contemporary policy debates about natural resource governance, environmental politics, and development aid.

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Chapter 1

How Corruption Drives Support for Fossil Fuel Subsidies: Evidence from Indonesia

1.1 Introduction

Governments in countries as diverse as China, Egypt, Indonesia, India, Nigeria, Russia, Togo, and Thailand, among others, subsidize the price of fossil fuel to consumers. Subsidies are not only economically expensive—on a post-tax basis, fossil fuel subsidies sum to nearly $2 trillion annually, or, 2.5 percent of global GDP—they are environmentally corrosive—eliminating fossil fuel subsidies would reduce global $C0_2$ emissions by 15 percent (IMF 2013: 1). Fossil fuel subsidies distort resource allocation by propping up energy-inefficient firms and industries. In low-income countries, fuel subsidies also tend to be reinforce inequality, by providing the urban, car-driving elite with greater benefits than poorer, rural households. Further, fuel subsidies impose significant financial strain on government budgets, crowding out spending on on priority policy areas like capital investments, health, education, and social safety nets. Given the economic and environmental costs of fossil fuel subsidies, the International Monetary Fund (IMF) has identified fossil fuel pricing reform as key to long-term fiscal stability (2013, 2014).

Despite the clear economic and environmental benefits that countries could reap by properly pricing fossil fuels, reform attempts often lead to popular resistance. Increases in energy prices triggered riots that ultimately toppled Indonesia's long-time dictator Suharto in 1998 and Kyrgyzstan's President Bakiyev in 2010. The Burmese government's announcement of fuel price hikes in 2007 led to widespread anti-governmental protests (Human Rights Watch 2007). A nationwide strike over the removal of fuel subsidies brought Nigeria to a standstill in 2012 and ultimately pushed the government to restore the subsidy. While many scholars make convincing economic and environmental cases for reducing energy subsidies (e.g. Arze del Granado, Coady and Gillingham 2012; Coady et al. 2006; Davis 2014; IMF 2013, 2014; IEA 2011; World Bank 2009), the question of how to reduce energy subsidies without causing social upheaval remains unresolved.

Why do citizens often reject reforms that can yield economic and environmental benefits? Do citizens really prefer spending on fuel subsidies to other forms of pro-poor and pro-growth spending? A common view is that citizen short-sightedness is both a cause of the initial policy—citizens demand income assistance and do not consider budget constraints (Sachs 1990)—and a cause of

failure to reform—citizens weigh the short-term costs of price adjustment more heavily than the long-term gains from more sound economic policies. With this in mind, some scholars advocate enacting pro-market reforms "by stealth" (e.g. Varshney 1998). Essentially, the argument goes, governments can successfully push through economic reforms while the public is "distracted" by issues like ethnic conflict. Even if this Machiavellian reform strategy were successful in some cases, it is least likely to succeed when reforming policies whose effects are direct, obvious, short-run, and widespread (ibid.). Removing subsidies on fuel prices affects citizens' consumption both directly (at the pump) and indirectly (in the cost of all consumer goods that utilize fuel as an input or incur transportation costs); its effect is obvious to citizens, who observe prices at the pump; and, the effect is both immediate and widespread. In short, it is hard to imagine an economic reform that is more necessary for governments' fiscal well-being and more susceptible to mass resistance.

Yet, citizens do not always reject subsidy reforms on basic commodities. Recent Afrobarometer polling finds that over 50% of citizens in African nations support market pricing on basic commodities like food and fuel (Bratton, Mattes, and Gyimah-Boadi 2005: chapter 5). Fuel subsidy reform was a key issue in Indonesia's 2014 presidential election. At the core of the debate was citizens' concern that the government's high spending on fuel subsidies–which amounted to over 25% of government expenditures in 2012–crowds out important long-term investments in education, health care and infrastructure. Citizens ultimately elected President Joko Widodo even as he promised to cut fuel subsidies to enable more pro-poor and pro-growth spending. Further, when President Widodo introduced fuel subsidy reforms after his inauguration, they were not met with the same mass resistance as similar reforms initiatives in Indonesia by the prior administration in 2012.[1]

The electoral promises made by President Widodo align closely with the prevailing policy wisdom on how to reform fuel subsidies without provoking mass resistance. Articulated most clearly

[1] Although President Widodo, widely known as "Jokowi", has been widely praised for eliminating fuel subsidies, the success of his initiative is yet to be determined. Jokowi did increase automotive fuel prices in December 2014, yet reduced them again in January 2015. Low oil prices mean that the market price for gasoline is now much closer to the administratively-set price of gasoline in Indonesia, but the price of gasoline is still set administratively. It will be difficult to tell whether or not Indonesia has successfully reformed the policy of subsidizing gasoline prices until market prices rise again.

by the IMF (2013), the prevailing policy wisdom is to replace spending on fuel subsidies with more direct spending on income assistance for the poor, like direct cash transfers. However, even when governments promise to replace fuel subsidies with more spending on the poor, citizens sometimes reject reform. In a reform initiative in Indonesia in 2012, then-president Susilo Bambang Yudhoyono similarly promised an expansion of spending on social safety nets, including on direct cash transfers and subsidized rice for the poor, in exchange for a 20% increase in the price of petrol at the pump, yet mass protests ultimately blocked the reform. Why did one reform initiative in the same country fail and another succeed, even though promises to supplant spending on fuel subsidies with compensation for the poor were similar?

This paper argues that, when citizens reject subsidy reform, they do not do so because they fail to value the potential benefits from shifts in government spending away from fuel subsidies and towards other forms of spending that better promote the welfare of the poor and the country's long-term economic growth. Instead, citizens reject subsidy reform when the promise to shift spending in ways that will actually benefit citizens lacks credibility. In the example above, President Widodo's unique reputation for being "apart" from the corruption that plagued former regimes no doubt played a role in enhancing the credibility of his reform promises.

This paper focuses specifically on the role that *local* governance institutions play in shaping citizens' attitudes towards reform. It does so, first, by noting that fuel subsidies are one of many potential tools for governments to redistribute wealth within society. Like other forms of social policy, fuel subsidies are extensive government interventions in the economy that reduce households' exposure to market risk and provide income assistance. Yet, energy subsidies are far simpler to administer than other, more efficient forms of income assistance. Delivering cash and in-kind benefits to the poor requires transferring valuable benefits through potentially corrupt local bureaucracies. By comparison, fuel subsidies are typically distributed by national oil companies, thereby bypassing bureaucracies and subnational governments altogether. Individuals can access the economic benefit of the subsidy directly at the pump, without having to appeal to local politicians and bureaucrats.

I argue that the tradeoff between economic efficiency and administrative simplicity is central to understanding public support for fossil fuel subsidies. On the whole, poor citizens prefer to receive income assistance through targeted social assistance programs, like cash transfers and targeted in-kind benefits. They do not universally support government spending on fuel subsidies: in fact, 39 percent of respondents in the sample wish to *decrease* government spending on fuel subsidies. When local politicians are corrupt, however, poor citizens are more likely to instead support fuel subsidies. Thus, the quality of local institutions forms a key barrier to increasing citizens' support for subsidy reforms.

To reveal these patterns, I rely on a combination of household survey data and administrative data from 572 villages across Indonesia. To measure local institutional performance, I utilize household-reported receipts of benefits from social assistance programs to estimate the total amount of program benefits accruing to households within a village. I then compare this with administrative data on the amount of benefits allocated to a particular village to derive an estimate for leakages in social assistance programs at the village level. Using this method, I estimate that 26 percent of the overall economic benefits from the primary social assistance program in Indonesia go missing. However, there is significant subnational variation in corruption: I estimate no corruption for nearly 30 percent of the villages in the sample, while a handful of villages experience program leakages close to 100 percent.

Variation in local institutional performance is strongly related to levels of support for energy subsidies. On the whole, poor citizens prefer the national government to allocate resources to targeted social assistance programs, like cash transfers and targeted in-kind benefits. However, when local politicians are corrupt and divert resources from these programs, poor citizens are instead more likely to support fuel subsidies, which are less vulnerable to diversions. This is true even if they do not own vehicles and do not directly purchase subsidized fuel.

However, it is possible that villages with greater local institutional corruption differ from less corrupt villages in ways which could also affect political attitudes. To alleviate this concern, I use matching methods to ensure comparability between villages with and without corruption in the

implementation of targeted transfer programs. This helps to confirm the causal interpretation of the results.

Throughout the paper, I focus primarily on the attitudes of poor citizens towards automotive fuel subsidies. I focus on poor citizens for two main reasons. First, politicians often justify failure to remove fuel subsidies out of concern for the poor, and it is worth understanding poor citizens' preferences about this type of spending. Second, the prevailing policy wisdom is to replace spending on fuel subsidies with direct cash transfers to the poor. This paper examines how poor citizens actually evaluate this tradeoff. This paper also focuses exclusively on automotive fuel subsidies, or, the retail price of gasoline at the pump, which is typically the most politically salient way that governments subsidize the consumption of fuel. And, in the case of Indonesia examined here, automotive fuel subsidies are the main target of political debates about the role of fuel subsidies in the budget.

This research extends and brings together three disparate literatures. First, it extends our understanding of the scope for costly reforms in low-income, democratic countries. Contrary to popular conceptions, I find that citizens do not universally support fuel subsidies and many are willing to support reduced government spending on them. However, support for fuel subsidies increases when the government's ability to redistribute wealth in other ways is compromised by lack of institutional capacity and corruption. This shows that the standard policy suggestion of enacting reforms by compensating the losers from the reforms may not be appropriate in countries where administrative capacity is low. When citizens do not trust that the government is capable of compensating them for losses, they are less likely to support the reform.

Second, this research expands our understanding of the link between local governance and national policy. A large literature explores the relationship between corruption and political attitudes by uncovering partial correlations between self-reported perceptions of corruption and attitudes (e.g. Anderson and Tverdova 2003; Chang and Chu 2006; Clausen, Kraay and Nyiri 2011; Seligson 2002). Yet, in decentralized countries, where subnational governments are responsible for implementing policies, the local political environment may substantially shape citizens' views of the

national system (Hiskey and Seligson 2003; Weitz-Shapiro 2008). I extend this logic by arguing that local institutional performance can also shape attitudes towards national *policies*. I improve inference by estimating corruption directly (rather than using self-reported measures) and by using matching methods. Understanding how individuals' actual experience with policy implementation feeds back into their support for social policies is also an important and relatively under-explored relationship in the literature on individual support for social policy (Campbell 2012).

Finally, this research contributes to our understanding of energy and environmental policy. Whereas there is a large and robust literature on the political effects of energy production and resource endowments (e.g. Colgan 2010; Haber and Menaldo 2011; Liou and Musgrave 2014; Rudra and Jensen 2011; Morrison 2009; Ross 2001, 2012), there remains "a surprising paucity of published work... on the politics of energy demand and supply management" (Hughes and Lipscy 2013: 452). The preferences of individual energy consumers remains a particularly neglected area (ibid.: 460). Yet, in developing countries with weak institutions, public attitudes can determine whether or not environmental policies are implemented at all (Greenstone and Hanna 2014). The research presented here shows that variation in institutional weakness within countries is also be an important *cause* of public support for environmental policy.

The paper proceeds as follows. Section 2 evaluates the literature on fuel subsidies and finds that the arguments for why politicians initially enact fuel subsidies do not shed much light on why fuel subsidies are so difficult to reform. To understand prospects for reform, we need to understand citizen attitudes. Section 3 provides an institutional theory of citizen attitudes towards fuel subsidies. Section 4 discusses the history of social policy and fuel subsidy reform in the Indonesian context. Section 5 reviews the research design, data collection, and measurement strategies for the key dependent and independent variables. Section 6 presents the results from the observational analysis, and Section 7 presents results from the matching analysis. Section 8 concludes.

1.2 Motivation: Why examine the demand side?

Subsidizing fuel consumption is one of many potential ways that the government can intervene in the economy to provide citizens with income assistance and protection against market risk. This paper focuses on consumer automotive fuel subsidies, governmental policies that lower the price of automotive fuels paid by end users.[2] Scholars propose a number of reasons for why governments may choose this policy over other means of distributing wealth. These explanations are rooted in supply- vs. demand-side arguments (Victor 2009). Supply-side arguments focus on the motivations of politicians to provide subsidies and tend to discount the role of citizen preferences in policy formation. Demand-side arguments emphasize citizen demand for fuel subsidies as a key motivation for government provision of fuel subsidies. While supply-side arguments provide insights into why governments initially choose fuel subsidies as a redistributive tool, they fall short in explaining why governments fail to change strategies once fuel subsidies become a fiscal burden. Demand-side explanations are better suited to shedding light on the prospects for reform, yet existing explanations may over-estimate citizen opposition to reform.

There are two main supply-side arguments for the prevalence of fuel subsidies across countries. For one, governments may be more likely to supply fuel subsidies in countries where energy is cheap and widely available. For countries that produce and refine energy domestically, the costs of the subsidy are born primarily as an opportunity cost (of not selling fuel products at market prices) and do not necessarily enter the budget as would other forms of spending. Further, the costs of providing the subsidy covary with the size of oil revenues: when oil prices are high, the cost of the subsidy rises, but so do oil revenues to the state. This eases the fiscal pressure of the subsidy. Empirical studies find that major oil producers are the most likely to subsidize fuel (Cheon, Urpelainen and Lackner 2013; Gupta et al. 2002).

Second, Victor (2009) argues that consumer fuel subsidies are a comparatively attractive tool

[2]The size of the pretax subsidy is the gap between the domestic retail price of the fuel product and the economic price of the product, which is determined by the wholesale price of the refined fuel product on the international market; the costs of transportation, distribution, and storage; and any profit margins by retail outlets. Thus, the cost of the subsidy for government budgets varies with world energy prices and, for countries that import refined fuel products for domestic consumption, with exchange rates.

for governments of countries that lack the administrative capacity to redistribute wealth by other means. Consumer subsidies on fuel are a highly visible means of distributing patronage to citizens, and they can be distributed using the existing capacity of national oil companies. These state-owned oil companies have been called a "state within a state" because they are often more capable actors than the state itself and perform many of the state's governance functions (Mommer 2002). Fuel subsidies in this view are an attractive policy tool to administratively weak states that have capable national oil companies. Empirically, Cheon, Lackner, and Urpelainen (2014) find that states with national oil companies are more likely to subsidize fuel.

Notably, Victor (2009) pushes the supply-side argument further, arguing that demand-side explanations are not fruitful for explaining the prevalence of consumer subsidies. Because consumers are a dispersed interest group, they are unlikely to overcome transaction costs to organize in favor of subsidies. He points to a "populist paradox": fuel subsidies are common amongst autocracies, not just democracies. Why would the regimes least susceptible to citizen input subsidize fuel if fuel subsidies were a function of citizen demand? Thus, he explains the use of subsidies as the choice of political leaders.

These supply-side explanations, however, do not yield much insight when considering why fuel subsidies are so difficult to repeal. Governments continue to fund subsidies even when energy is no longer cheap compared to other forms of wealth distribution. As declining production and growing domestic consumption of energy resources push countries like Indonesia from being an energy exporter to an energy importer, countries frequently fail to roll back subsidies. Energy exporters that lack refining capacity also have to import refined fuel products at market prices, meaning that even net energy exporters can face intense fiscal pressures to reform. Nigeria, for example, is an oil exporter yet imports over 85 percent of the refined fuel products consumed domestically (Siddig et al. 2014). Despite intense fiscal pressure to reform, Nigeria remains a large fuel subsidizer. While the relative abundance of cheap energy may initially motivate major oil producers to utilize energy subsidies as a means of redistributing wealth, this does not explain why these countries fail to shift strategies as the fiscal burdens of the subsidy rises. Further, if fuel

subsidies were primarily a function of politician-demand, then why do politicians attempt, and fail, to reform? Politicians have tried to reduce fuel subsidies—lending evidence that political leaders funding fuel subsidies do not always want them in place—only to be blocked by strong protest and opposition from citizens. During the 2012 oil price increase alone, the governments of Indonesia, Nigeria, Sudan, and Yemen all attempted fuel subsidy reform. In the face of strong opposition and protests, all four attempts were reversed.[3]

Given the role of protests in reversing reform attempts, understanding when and why citizens demand fuel subsidies is essential to evaluating prospects for reform. The literature on citizen-state relations in oil-producing countries offers a more citizen-centric explanation for the prevalence and persistence of fuel subsidies. Classic descriptions of the *rentier* state depict a kind of oil-based social contract, under which politicians utilize patronage and highly visible public spending projects, of which subsidized fuel is a prominent example, to relieve pressures for greater accountability (e.g. Beblawi and Luciani 1987; Mahdavi 1970). As long as politicians continue to deliver patronage, citizens do not hold governments accountable for poor performance. However, the literature on resource-rich states also notes that citizens can over-demand patronage relative to government resources. Resource revenues foster "a distinctive form of 'fiscal illusion' whereby the perception that someone... else is shouldering the tax burden reinforces societal support for unbridled state spending" (Jones Luong and Weinthal 2010: 65). In other words, citizens care less about the efficiency of public spending and government performance so long as they receive visible benefits from resource wealth. Fuel subsidies are a particularly visible way of delivering these benefits. Based on this logic, Ross (2012: ch 3) offers a different explanation for Victor's "populist paradox." He argues that autocratic regimes may be more susceptible to citizen protests against the removal of fuel subsidies. They keep fuel subsidies in place because they fear being overthrown. In these accounts, governments continue to provide fuel subsidies even when their fiscal costs grow because citizens in resource-producing countries have unrealistically high demands from government budgets and failure to meet these demands poses risks for politicians.

[3]Indonesia initiated other reform efforts in 2013 and 2014 which were successful in raising government-set fuel prices, though the government continues to administratively set the price of automotive fuel at the pump.

Yet, even in resource-rich states, there is a high fiscal opportunity cost to spending on fuel subsidies. Indonesia is a prime example. The Indonesian government spends more on fuel subsidies than on education, health, and social protection combined (IISD 2012). Do citizens really not want greater spending on these priority areas? Or, are citizens insensitive to the idea of budget constraints? We widely expect citizens to reject pricing reform, despite the fiscal and environmental costs of subsidies. Yet, according to Afrobarometer, a *majority* of citizens of African countries support paying market prices for basic commodities to reduce shortages, even though market prices will be higher than government-administered prices (Bratton, Matte, and Gyimah-Boadi 2005: ch 4). We know relatively little about citizens' policy preferences and the sort of tradeoffs they find acceptable with regards to fuel subsidies, but there may be more flexibility in citizen attitudes towards basic commodity subsidies than is typically assumed.

Why do we assume that citizens support government spending on subsidized fuel products at the expense of investments in education and health care? And, to what extent is this assumption true? In accordance with the idea that understanding citizens' attitudes towards reform is an important precursor to understanding governments' scope for reform, the rest of the paper offers and tests a theory for why citizens support fuel subsidies.

1.3 An institutional explanation for citizen attitudes

This paper argues that local institutions are key to shaping citizens' attitudes towards fuel subsidy reform. Where local institutions perform poorly, citizens are less likely to trust the government and less likely to believe that increases in governmental spending in other areas will ultimately benefit them. I note that the economic benefit from fuel subsidies is delivered to citizens in a fundamentally different way compared to other governmental policies. Because fuel subsidies are delivered outside of typical bureaucratic channels, they are particularly valuable to citizens who live in areas where those bureaucratic channels are weak.

The central government must delegate a range of governing activities, from tax collection to policy implementation, to local bureaucrats on a daily basis. Whenever governing authority is delegated, it opens up the potential for local bureaucrats to exploit their position, diverting public resources for private gain. Where corruption in local institutions interferes with the delivery of services, it opens up a gap between commitments of resources at the national level and delivery of services at the local level. Potential for corruption is particularly high in the case of targeted assistance programs. Constraints on the ability to find beneficiaries and deliver benefits to them often requires the involvement of local officials, giving them implicit control over program implementation (Jaspars and Shoham 1999). Thus, while decentralization has the potential in theory to improve service delivery by bringing responsibility for service delivery closer to users, it can result in over-providing benefits to local elites at the expense of the poor when local elites are "captured" (Bardhan and Mookherjee 2005). Malesky, Nguyen, and Tran (2014) find that the primary benefit of recentralizing public service delivery in Vietnam was removing corrupt local councils from the policy-making process. Corruption in the implementation of targeted transfer programs in particular can be so costly that it eliminates the potential benefits from redistributive policies (Olken 2006).

However, decentralization of service delivery does not automatically lead to corruption. Even in countries widely perceived as corrupt, there is significant subnational variation in the extent of corruption (see Golden and Picci 2005 on Italy; Olken 2006 on Indonesia; and Reinikka and Svensson 2004 on Uganda). Thus, an important prerequisite for testing the relationship between corruption and attitudes is establishing the level and variation of corruption in the sample.

Where corruption corrodes public service delivery, citizens can lose trust in government. By contrast, when institutions achieve high policy performance, citizens can reward governments with trust. The idea that citizens' trust in government is based on their evaluation of the institutional performance is supported by a large literature on the effects of corruption on political trust (Anderson and Tverdova 2003; Chang and Chu 2006; Easton 1965; Ellinas and Lamprianou 2014; Hakhverdian and Mayne 2012; Seligson 2002). However, citizens do not just make their per-

formance assessments based on the actions of the national government. Instead, the majority of citizens' direct interactions with the state occur locally, with local politicians and bureaucrats. This is especially true when service delivery is heavily decentralized. Thus, the performance of local institutions in particular can influence attitudes towards political systems (Hiskey and Seligson 2003; Weitz-Shapiro 2008).

I extend the logic that institutional performance shapes citizens' attitudes towards government by arguing that institutional performance also shapes citizens' attitudes towards the policies delivered by those institutions. The quality of local institutions shapes citizens' experiences with government services, like education, health care, and targeted social benefits. However, the quality of local institutions notably does not shape citizens' experiences with fuel subsidies. Fuel subsidies are not delivered through typical bureaucratic channels. Instead, fuel subsidies are usually delivered by national oil companies directly to fuel distribution points (e.g. gasoline stations), and access to the subsidy is universal. Citizens' ability to access the economic benefit from the subsidy thus cannot be restricted by local politicians. Universal and transparent access to an economic benefit can be particularly valued when the criteria to access to targeted programs are opaque and access is controlled by politicians. Even if the institution that delivers the fuel subsidies is itself corrupt, this high-level corruption rarely restricts citizens' ability to access the subsidy or the size of the economic benefit that they can derive from the subsidy.[4] This is in contrast to when there is corruption amongst the local politicians through which the national government delivers the benefits from a direct cash transfer; in this case, corruption directly shapes citizens' experience with the program.

Trust in local institutions is particularly important when citizens are considering prospective economic reforms. Where the quality of local institutions and local service delivery is high, I argue, citizens are more likely to accept fuel pricing reform, because the political promise that savings

[4]In Indonesia, subsidized fuel is frequently smuggled out of the country and sold at international prices (the subsidized price of fuel in Indonesia is only 50 percent of the cost of fuel in neighboring Malaysia and Singapore) (Lingga 2014). Indeed, when the Suharto regime initiated fuel price hikes during the deregulation of the 1980s, the public saw price hikes as evidence of Pertamina's mismanagement and corruption. In other words, low fuel prices were seen as a *check* on corruption (Soesastro 1989).

from reduced spending on fuel subsidies will translate into long-term benefits for citizens is more credible. These citizens are more accustomed to benefitting from governmental spending and have higher levels of trust in government. By contrast, citizens that experience poor local institutions and service delivery are less likely to believe that any savings the government gets from reducing fuel subsidies would improve their welfare. Where local service delivery is poor, citizens are more likely to see fuel pricing reform as a net negative: they may lose the economic benefit they derive from the subsidy without gaining any benefits from potential increases in governmental spending on programs delivered through local institutions.

In sum, corruption shapes attitudes towards fuel subsidy reform by affecting governments' ability to effectively deliver income assistance by other means. Where local officials fail to deliver benefits from targeted assistance programs, households that would otherwise support more efficient forms of government spending instead support fuel subsidies. Fuel subsidies are comparatively attractive to citizens living in areas where local officials are corrupt because they are delivered outside of typical bureaucratic channels. I expect that corruption will primarily affect the attitudes of those that directly experience corruption (i.e. the poorer households that are the intended beneficiaries of government transfers). This paper provides a test of this argument.

1.3.1 *Rival Explanations and Confounds*

This paper argues that corruption reduces trust in the local institutions that deliver social assistance programs and, through this mechanism, increases their support for government spending on fuel subsidies. However, an observed effect of corruption on attitudes could function through a different mechanism (other than or in addition to trust in local institutions) or it could arise because another factor simultaneously shapes both corruption and attitudes.

Corruption could shape attitudes through an economic self-interest rather than a trust mechanism. While I argue that corruption shapes political attitudes towards government spending primarily by affecting citizens' trust in the local institutions that deliver social assistance programs,

corruption could also affect attitudes by reducing the economic value that they receive from social assistance programs compared to the economic value received from fuel subsidies. Thus, it is possible that citizens do not care about corruption *per se* but care about the level of economic benefits they receive from a given program. Corruption could shift the relative economic value of programs, causing citizens to shift support from targeted social assistance programs to fuel subsidies. If this is the case, then we should expect the effect of corruption to be stronger among those that derive greater material benefits from fuel subsidies. I test for this possibility.

Both corruption and attitudes could be also shaped by other factors. I identify three primary sources of confounds. First, village remoteness could cause both support for fuel subsidies and corruption. The economic value of the fuel subsidy varies based on the varying economic costs of fuel. Thus, the economic value of a fuel subsidy (on a per unit basis) is generally higher in more remote areas where the costs of transporting and distributing fuel are higher. Remote areas are thus hardest hit when fuel prices increase, primarily through increases in the costs of other goods which now face higher transportation costs. Alternatively, we know that households in urban areas consume higher absolute levels of fuel and, thus, that support for fuel subsidies is higher in urban areas. Either way, village remoteness may cause attitudes towards subsidies. Further, remoteness could cause corruption. Again, the effect could flow in either direction. More remote villages are more difficult for the central government to monitor, opening up more opportunities for corruption. More remote villages also tend to be smaller, and they may have more social capital compared to urban areas, reducing corruption.

Second, local political institutions could shape both attitudes and corruption. There is heterogeneity in Indonesia in the method of selecting local officials for political office. In some villages (*desa*), the village head is elected by the local population, while in others (*kelurahan*), the village head is appointed by the district mayor. Martinez-Bravo (2014) finds evidence that appointed officials tend to be more active in electoral fraud; it is possible that other forms of corruption are higher in these villages as well. Electing village officials also gives the local population the opportunity to throw out corrupt incumbents, providing a check on local corruption. These local institutions

could plausibly affect political attitudes towards national government spending, too. Improved local accountability could increase support for government programs that rely on local officials for implementations, since voters have more guarantees that they will be able to punish officials to administer the programs poorly.

Third, local culture could confound analysis. Villages with higher social capital could be better able to coordinate around punishing corrupt local officials, and social capital could encourage support for government programs that provide more diffuse social benefits. Indonesia is also famously heterogeneous both culturally and linguistically, and broad cultural differences could shape corruption and attitudes.

I address these potential confounds in several ways. I control for village remoteness, local political institutions, and social capital in the analysis. If these factors just shape attitudes, then the analysis of the effect of corruption on attitudes in a normal regression framework should be fine. If they also cause corruption, however, then there could be bias in the corruption estimate. Thus, I use propensity score matching to ensure that corrupt and uncorrupt villages are similar along these characteristics. Matching within districts helps to ensure that unobservables that are spatially correlated, like culture, are not biasing results.

1.4 Fuel subsidies in Indonesia: an imperative to reform

Fuel subsidies were initially put in place in Indonesia under the Sukarno administration in the 1960s. They were reportedly intended to protect the poor from inflation, which reached as high as 500 percent during the period (Beaton and Lontoh 2010). When Suharto assumed power in 1968, consistent with his embrace of liberal economic theory, he reduced the subsidies on fuel. However, the government continued to administratively set the prices for fuel products (Beaton and Lontoh 2010). The Suharto regime adjusted fuel prices several times to curb spending during its 31-year rule, with the most dramatic adjustments coming during the broad deregulation initiatives of the 1980s (Soesastro 1989). Despite these adjustments, domestic retail fuel prices were never linked

to market prices.

However, fuel subsidies never imposed significant financial strain on the government until the Asian Financial Crisis hit Indonesia in 1997. Among many other devastating economic impacts, the sharp devaluation of the Indonesian rupiah during the crisis caused the fiscal cost of fuel subsidies to escalate tenfold, from 0.3 percent of GDP in 1996 to 2.9 percent of GDP in 1998 (Clements, Jung, and Gupta 2007: 222). As for many developing countries facing financial crisis during the 1980s and 1990s, the World Bank and International Monetary Fund (IMF) provided large loans to the Indonesia government in return for commitment to implement policy reforms, including greater austerity. One of the lending conditions was a severe reduction in government subsidies on fuel. In early May 1998, Suharto therefore announced a 71 percent increase in the price of petrol. This announcement served as the trigger for the widespread and violent riots that ultimately pushed Suharto to resign from office in late May 1998, ending his 31-year rule. The double shock of the Asian Financial Crisis and the reduction in basic commodity subsidies was devastating for Indonesia's poorest citizens: the poverty count in Indonesia doubled from 23 million in 1996 to almost 50 million in 1998 (World Bank 2012a).

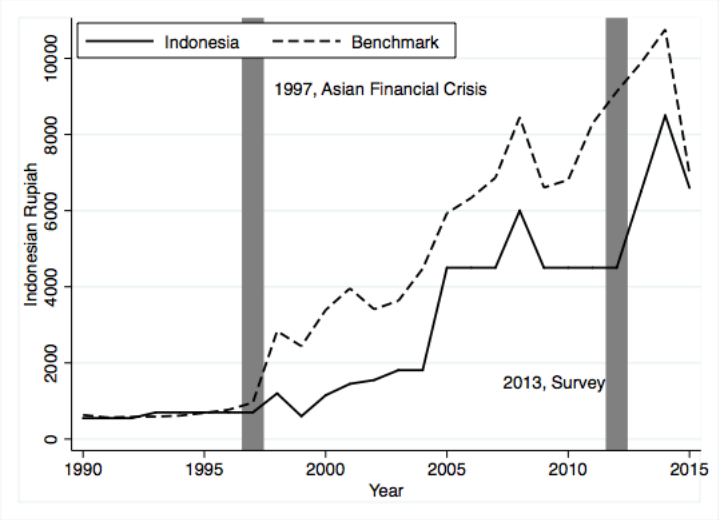
To address the poverty crisis in Indonesia, newly-inaugurated President Habibie moved quickly to initiate Indonesia's first social safety net programs. Most prominently, the government introduced *Beras Miskin* ("Rice for the Poor"), commonly called Raskin, a subsidized rice program targeted to poor households, just a few weeks after Suharto's resignation.[5] To date, Raskin is still Indonesia's largest social assistance program, accounting for 53 percent of all social expenditures (Government of Indonesia 2012). The government framed Raskin as a means to help the poor cope with the financial crisis and with higher fuel prices.

Like Raskin, nearly all social assistance programs in Indonesia were initially implemented to coincide with government-imposed hikes in the price of fuel. In 2005, the government introduced a health fee waiver for the poor and a temporary, unconditional cash transfer program to compensate poor households for the effects of nearly doubling the price for gasoline. In 2008, the

[5]The government also introduced a temporary health fee waiver program for the poor, which was not widely implemented and phased out within a few years (Rosser 2012).

government again raised gasoline prices by another 29 percent and again implemented the temporary, unconditional cash transfer program. In mid-2013, President Yudhoyono again raised the price on subsidized fuel, accompanied by another temporary, unconditional cash transfer to poor households and a doubling of Raskin benefits. In each of these cases, increases in social welfare spending were clearly and publicly linked to decreases in spending on fuel subsidies. Thus, in Indonesia, citizens have a relatively high level of awareness about the tradeoffs between spending on social safety net programs for the poor and spending on fuel subsidies.

Figure 1.1: Indonesia Government Expenditures, 2012



Sources: Beritasatu.com, World Development Indicators, Energy Information Administration

Figure 1 shows a history of administratively-set gasoline prices in Indonesia against an international benchmark price.[6] The gap between the benchmark and the administratively-set price of gasoline in Indonesia represents the approximate size of the subsidy.[7] The figure illustrates that fuel subsidies indeed begin to become a sizable expenditure for the government of Indonesia after the Asian Financial Crisis and the devaluation of the rupiah. The figure also illustrates a number of the reform events highlighted above, where the government of Indonesia raises the price of gasoline at the pump. Differences between domestic prices and benchmark prices are driven

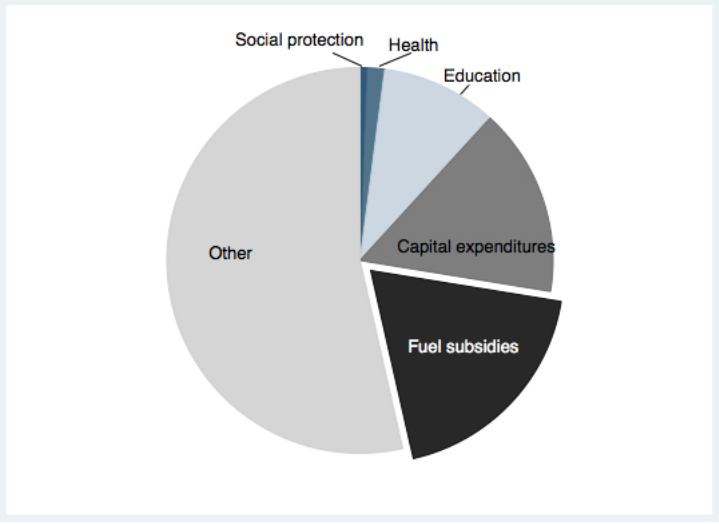
[6]I utilize the average price of gasoline in the U.S. as a benchmark, as this is frequently used as an international benchmark price for gasoline since the U.S. imposes low taxes on the consumption of gasoline.

[7]The figure does not account for differences in transportation costs between Indonesia and the benchmark. In reality, the size of the subsidy is likely larger than is depicted here.

by the administratively-set price of gasoline, exchange rates, and changes in the market price for gasoline. Also highlighted on the figure is the price of gasoline in Indonesia in March 2013, when the survey used here was fielded. At the time, the price of gasoline in Indonesia was around $1.60 per gallon, while the benchmark price was around $3.65 per gallon.

Figure 2 illustrates how the subsidy translates into government spending. Despite the reform efforts over the past decade, fuel subsidies in Indonesia remain substantial. In 2012, the government of Indonesia spent around US$14.5 billion on fuel subsidies, accounting for around 13 percent of government expenditures (IISD 2012: 5). This number is even higher when you include electricity subsidies. Both fuel and electricity subsidies accounted for around 20 percent of government expenditures in 2012, and climbed to around 25 percent of expenditures in 2013 (ibid.). High international crude prices, a weak Indonesian rupiah, and the exponential expansion of domestic energy consumption contribute to the enormous cost of the subsidies. The opportunity costs of the spending are staggering: spending on fuel subsidies exceeded spending on capital expenditures by 10 percent, a fundamental driver of long-term growth, and spending on eduction, health care, and social protection combined (see Figure 2).

Figure 1.2: Indonesia Government Expenditures, 2012



Source: Government of Indonesia, 2012.

Before proceeding, it is worth understanding who the primary beneficiaries of fuel subsidies

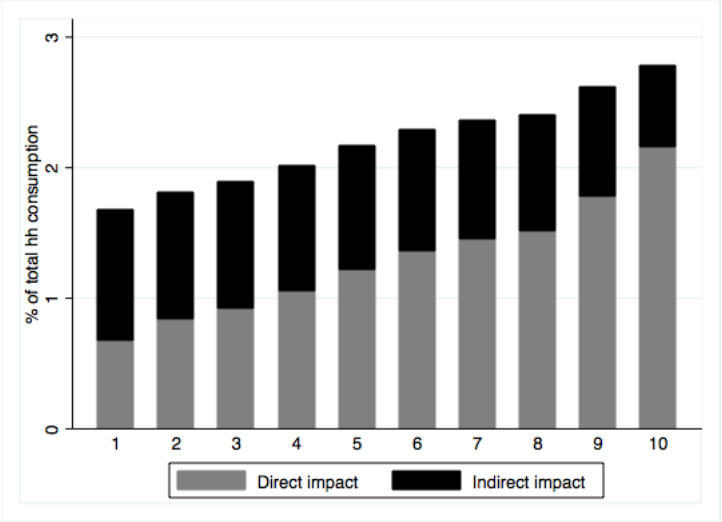
within Indonesia are. Although the richest households are the primary consumers of fuel, poor households in Indonesia are increasingly becoming direct consumers of automotive fuel due to expansion in motorbike ownership. In the survey sample used here, 40% of poor households[8] report owning vehicles. Figure 3 calculates the effect of a 25% fuel price increase on household welfare by household consumption decile using the survey sample collected for this project.[9] The direct impact of a fuel price increase is the product of the projected fuel price increase by the share of fuel costs in household consumption. For the purposes of this analysis, the indirect impacts of fuel price increases have been vastly simplified as the effect of a fuel price increase on the price of food. In reality, the price of fuel would also enter into many non-food consumption goods, and households could respond to price increases by substituting across products. I assume that a 25% increase in the price of fuel leads to a 1.5% increase in the price of food.[10] Figure 3 shows that poorer households are less affected by fuel price hikes than richer households, consistent with the fact that richer households consume more fuel directly. However, poor households are clearly benefitting economically (both directly and indirectly) from fuel subsidies and experience negative welfare effects from price hikes. This analysis merely shows that all households in Indonesia have an economic stake in fuel subsidy reform and not just the rich. All analysis going controls for vehicle ownership and fuel costs as a share of household consumption.

[8] Defined as the 30% of the households in the country with the lowest consumption levels.

[9] The analysis employs entropy balancing to reweight the survey sample to reflect national demographics, discussed below.

[10] This assumption is based on analysis done in Arze del Granado, Coady, and Gillingham (2012) on the indirect effects of fuel price increase (see Table 10).

Figure 1.3: Economic impact of a 25% fuel price increase by consumption decile



Source: Authors' calculations

Attempts to compensate poor households for the welfare losses from fuel price reform through new social programs have been implemented imperfectly. For one, overall spending levels on social assistance programs are low. Public expenditures on social assistance programs account for only 0.5 percent of GDP, in comparison with other developing countries that spend an average of 1.5 percent of GDP on social assistance (World Bank 2012a). Second, because it is difficult to directly observe household income, the government determines eligibility for social assistance programs using proxy-means tests (PMT). Alatas et al. (2012) find that PMTs resulted in erroneously excluding 53 percent of poor households from social program eligibility and including 20 percent of non-poor households.[11] Thus, from the perspective of poor citizens, it may be difficult to anticipate whether they will receive benefits from social programs or not, and the criteria for inclusion seem opaque. Third, social assistance programs are delivered through local bureaucrats, who may be corrupt. Indeed, Olken (2006) finds that corruption in the delivery of Raskin entirely offsets the redistributive value of the program. This is in accordance with much of the literature on politics in contemporary Indonesia, which argues that the centralized corruption of the Suharto era has been replaced with pervasive, decentralized corruption within local governments (e.g. Hadiz

[11]Note that this is still significantly better than random targeting as the poor represent only 30 percent of the population.

2010). The remainder of the paper explores how this corruption within local governments shapes citizens attitudes towards the prospect of fuel subsidy reform.

1.5 Data and Measurement

1.5.1 *Data Collection*

The data presented here was collected as part of a project conducted jointly with the Government of Indonesia designed to reduce leakages and corruption in the Raskin program (Banerjee et al. 2015). Raskin is designed to provide 15 kg of heavily-subsidized rice to eligible households, who represent the poorest 27 percent of the population. With an annual budget of US$1.5 billion, and a targeted population of 17.5 million households, it is Indonesia's largest targeted transfer program. The program is also substantial from the beneficiary's perspective: the subsidy provided through the program equals around 4 percent of the average beneficiary household's monthly consumption.

Eligible households, however, do not necessarily receive those benefits. The process by which households receive subsidized rice illustrates how local officials can divert resources for private gain. Each month, the village government is responsible for picking up the village's allotment of rice (15 kg per month for each eligible household in the village) from a government warehouse, usually located in the sub-district capital. Village governments are then responsible for dividing the rice (usually received in 60 kg packages) into 15 kg sacks, for distributing it among the targeted beneficiaries within the village, and for collecting copayments. The village government is then responsible for remitting the copayments back to the government logistics agency that coordinates rice distribution nationally. This payment from the village government to the logistics agency is the only form of top-down monitoring over the program.

Local officials have two opportunities to divert program resources for private gain: (1) they can divert a portion of the village's rice quota to sell on the side at market prices (which are on average 5 times higher than the official copay price) and (2) they can sell Raskin rice to eligible households

within the village at a price higher than the official copay price and pocket the difference. The first amounts to diversion along the quantity dimension and would be reflected in lower rice purchases by eligible households. Olken (2006) estimates that at least 18 percent of rice goes missing in this manner, before it ever reaches villages, a cost that offsets the redistributive benefits of the program. The second amounts to diversion along the price dimension and would be reflected in higher Raskin purchase prices among households within the village. Fieldwork we conducted in 2012 indicated that, anecdotally, substantial leakage occurs along the price dimension as well. In one village, the value of the price mark-up equaled around US$30,000 annually. In this sample, households report on average paying a copay price 40 percent higher than the official copay price.

The data presented here is based on two surveys administered by SurveyMetre, a survey company based in Indonesia.[12] The sample consists of six districts, two districts each in the provinces of Central Java, Lampung, and South Sumatra. These districts provide important heterogeneity in institutions and culture, particularly by being located on- and off-Java. Within these districts, we randomly sampled 58 sub-districts (*kecamatan*). For each sampled sub-district, we included all villages in the study ($n = 572$).[13] These 572 villages comprise the final survey sample. We conducted two surveys in these villages: one in October-November 2012 and one in March-April 2013. For each round of surveying, we randomly selected a hamlet within the village. For the first round of surveying, we targeted 8 households per village. For the second round of surveying, we targeted an average of 10 households per village, depending on geographic area. During each survey, we randomly sampled from two overlapping sampling frames. First, we randomly sampled from a full hamlet census (on average, 70 households). Second, we randomly sampled households from the official government registry of Raskin-eligible households, which roughly corresponds to the poorest 27 percent of the population.[14] This is the same list of households that are eligible for all

[12] SurveyMetre was selected through an official public procurement process conducted by the Government of Indonesia. They are also the survey company that conducts the Indonesian Family Life Survey (IFLS).

[13] There were originally 600 villages in the sample, but 28 were dropped due to safety concerns.

[14] We later machine-matched the households randomly-sampled from the census to the registry of Raskin-eligible households so that we could fully identify which households in the sample were eligible for targeted transfer programs in the sample. Around 9 percent of the households randomly-sampled from the census were reclassified as Raskin-eligible using this method.

targeted assistance programs in Indonesia, including the unconditional cash transfers granted when the government imposes fuel price hikes. In both surveys, we asked questions about household rice purchases and household economic status. Questions about attitudes on government spending were only asked in the second round of surveying.

While this sample is not nationally representative, sample demographics are similar to national averages (Table 4, Appendix). Compared to national averages, the sample is slightly skewed towards rural areas, and, despite oversampling the poor by sampling directly from government registries of poor households, contains fewer households in the lower consumption deciles. To correct for sampling imbalances, I use entropy balancing to reweight the survey data to match demographic information from the population (Hainmueller 2012; Hainmueller and Xu 2013). Table 4 (Appendix) presents demographic information in the weighted and unweighted data.

1.5.2 *Research Design*

I investigate the relationship between corruption in the implementation of targeted transfer programs and support for fuel subsidies in two stages. First, I examine the relationship between level of corruption in the implementation of Raskin and support for fuel subsidies in an observational study, examining only villages randomly-assigned to the control group in the field experiment (n=194). This analysis enables me to test whether the existence and level of corruption in Raskin is correlated with individuals' support for fuel subsidies. I control for a battery of household and village characteristics in the analysis.

The idea that corruption diminishes trust in governments has been extensively explored in the literature, yet it is difficult to establish a causal relationship between corruption and attitudes. Corruption is notoriously difficult to measure, primarily because corrupt officials have strong incentives to conceal their activities. Thus corruption is often measured based on citizens' perceptions of corruption. Several studies document a negative partial correlation between perceptions of corruption and confidence in public institutions cross-nationally (e.g. Anderson and Tverdova 2003) and at a household-level (e.g. Chang and Chu 2006). However, it is possible that the same underlying characteristics causing individuals to report high levels of corruption also cause them to

report low confidence in public institutions. Indeed, Olken (2009) finds that while there is a strong correlation between perceptions of corruption and real levels of corruption, individuals are biased in their perceptions.

To mitigate endogeneity concerns with using perception-based corruption measures, other studies utilize self-reported data on personal experiences with corruption (e.g. Clausen, Kraay and Nyiri 2011; Seligson 2002). Although it may seem like using experience-based measures of corruption alleviate endogeneity concerns, there is still the larger issue that unobserved factors might cause both corruption and attitudes. Personal experiences with corruption can be distributed among the population in non-random ways. This would be true, for example, if both levels of corruption and attitudes varied systematically within countries, caused, say, by variations in local political culture. Self-reported experiences with corruption can also be subject to personal bias related to political attitudes.

This paper takes endogeneity concerns seriously by making two methodological improvements on previous work on the effect of corruption on political attitudes. First, rather than relying on self-reported perceptions of or experiences with corruption, I estimate corruption at the village level using a "gap measurement" method. This ensures that estimates of corruption are not subject to bias and are not systematically related to individual characteristics. Although this improves inferences from past studies, it is still possible that other village-level characteristics cause both political attitudes and corruption.

In order to strengthen my argument that the relationship between corruption in local governance and political attitudes is a causal one, I use matching methods to ensure that corrupt and uncorrupt villages are comparable in a second set of analyses.

1.5.3 *Dependent Variable: Opposition and Support*

Our survey asked about attitudes towards social policies across multiple policy domains using two main questions. The first asked respondents whether funding should be increased for existing social policy programs in eight areas: direct cash transfers, subsidized rice, health fee waivers, hiring teachers, improving roads in rural areas, community-driven development funds, reducing

the price of automotive fuel (gasoline and diesel)[15], and reducing the price of LPG gas.[16] These policies were selected as the primary ways that the Government of Indonesia allocates budget to promote the welfare of poor citizens. The second question asked respondents whether funding should be decreased for the same set of policies. Question wording was similar to that used by Rehm, Hacker and Schlesinger (2012) in their cross-domain analysis of social policy support in the United States.[17]

I measure support for automotive fuel subsidies using a three-category outcome variable: where "1" indicates that the respondent selected to increase spending on fuel subsidies, "0" indicates that the respondent selected neither to increase nor decrease spending on fuel subsidies, and "-1" indicates that the respondent selected to decrease government spending on fuel subsidies. I focus on support for automotive fuel subsidies (rather than on cooking fuels like LPG) because these are the most environmentally and fiscally damaging form of fuel subsidy; they are also the largest in terms of government budget and the most salient to citizens.[18] My argument about fuel subsidies bypassing local institutions is also less applicable in the case of cooking fuel subsidies. LPG subsidies are distributed through a combination of public and private channels, and the price to users is set by distributors. This makes the subsidy less obvious to end users and more susceptible to local price mark-ups. There has also been the most political debate about the role of automotive fuel subsidies in the government budget. In 2012, 2013 and 2014, the government attempted (in 2012, unsuccessfully, and successfully in 2013 and 2014) to increase the price of subsidized automotive fuel. Debates about whether and when to reduce automotive fuel subsidies was also a key topic in debates leading up to the 2014 presidential election in Indonesia. These political

[15]Subsidies on both gasoline and automotive diesel are collectively called *Subsidi BBM* in Indonesia, a widely used and understood term.

[16]Question wording: "The government has a number of programs to help the poor. Imagine that you could plan the government's budget this year. If the government could increase [had to decrease] the budget for only three of the following programs, which programs would you select?" [eight potential answers].

[17]It is worth noting that the question primes the idea of "programs to help the poor." Thus, it may be difficult to tell whether the respondent is answering the question from the perspective of which programs benefit them as individuals compared to which programs benefit the poor collectively. However, there is unlikely to be much difference between these interpretations in this sample as 96% of respondents self-identify as being "less well off" compared to others.

[18]Indonesia has already made significant progress in phasing out environmentally harmful kerosene subsidies, see Pertamina (2012).

debates hinged on the financial strain imposed by automotive fuel subsidies (e.g. Suryowati 2014) and the potential to increase spending on pro-poor policies and economic development if spending on automotive fuel subsidies were reduced (e.g. Rakhma and Setiawan 2014). Thus, in the rest of the paper, when I use the term "fuel subsidy," I am referring specifically to automotive fuel subsidies in Indonesia and not cooking fuel subsidies.

The question was designed to elicit a ranking of respondents' preferences towards various forms of government spending while eliciting the idea of a budget constraint. In theory, a respondent could want to increase government spending on all policy areas. By limiting the respondent to three policies on which to increase government spending and having the respondent additionally select three policies on which to decrease government spending, we can get a sense of their most and least-preferred policies. So, I interpret a respondent selecting to increase spending on automotive fuel subsidies as having a greater preference towards government spending on automotive fuel subsidies than a respondent selecting to increase government spending on other policy areas and greater still than a respondent selecting to decrease government spending on automotive fuel subsidies.[19]

To ensure that a respondent who strongly prefers spending on social assistance programs to spending on fuel subsidies could be accurately represented, I included the three largest targeted transfer programs–Raskin, cash transfers for the poor, and health fee waivers for the poor–in the question. These policies receive comparatively low levels of baseline support from the government (see Figure 2). Further, it is possible that a respondent prefers to receive income assistance through fuel subsidies but does not support automotive fuel subsidies, which have the greatest fiscal and environmental costs. Including LPG subsidies as a spending category allows households that value fuel subsidies to select greater spending in this area while still not selecting to increase spending on

[19]We piloted several versions of this question to ensure that respondents understood the question and that it was successful in eliciting political attitudes. Initially, the question asked individuals to rank policies by the degree to which they wished to increase or decrease government spending. However, the ranking exercise proved to be too complicated, and respondents did not always have a clear ranking of preferences over all eight programs. However, respondents could readily provide the three programs on which to increase and three programs to decrease government spending. I thank SurveyMetre, especially Cecep Sumantri, and J-PAL Southeast Asia, especially Nurzanty Khadijah, for piloting the questions and providing valuable input.

automotive fuel subsidies. Finally, I include three categories of public goods spending–community development funds, building roads, and hiring teachers–to give options to those respondents preferring spending on public goods to targeted assistance programs and to fuel subsidies.

It is worth noting that supporting increased levels of spending on a policy is not the same as supporting a policy generally, nor is supporting decreased levels of spending on a policy equivalent to policy opposition. For example, an individual may prefer high levels of government spending on education in a hypothetical world but in reality think that current forms of government spending on education are ineffective. Therefore, on a question about how to allocate government spending, they may select to decrease government spending on education, even if they prioritize education as a policy area. Or, an individual may select neither to increase nor decrease government spending levels on a high priority policy domain because they are already satisfied with spending levels. By the same token, an individual's selection to decrease government spending on fuel subsidies could reflect that the individual would still like to receive a sizable fuel subsidy but not quite as sizable as the status quo.

Preferences for more or less policy spending more accurately represents the difference between an individual's preferred level of policy spending and the baseline level of policy spending (Wlezien 1995). I therefore interpret individuals' responses to the survey question as the disjuncture between their preferences and status quo spending levels, given existing realities about the quality of policy implementation, and not as either an overall indicator of support for a given policy. However, for convenience, I use the terms "support," "neutral" and "oppose" in the discussion of results to indicate whether the individual selected to increase, neither to increase nor decrease, or to decrease government spending on gasoline subsidies.

These types of spending questions can be difficult to interpret in a cross-national setting, as they ask people about spending preferences relative to different baseline spending levels. All of the factors that purportedly explain differences in spending preferences also vary across countries in addition to the variation in baseline spending levels across countries. However, this should not be a concern when looking across policy domains within a single country. By looking at spending

preferences within a single country, I implicitly hold constant a wide range of factors that are argued to cause cross-national divergences in spending preferences, including the overall structure of government spending, popular attitudes, distribution of income and skills, and any legacies from past policies that affect all citizens equally. This reduces concerns about confounders and enables me to focus on how subnational differences in institutional performance in the implementation of social policy shape attitudes.

1.5.4 *Estimating Corruption*

I use a "gap measurement" method to estimate corruption, which estimates corruption by identifying discrepancies between different data sources. This paper uses a method developed and applied in Olken (2006), which estimates corruption in Raskin by comparing administrative data on village rice quotas with household survey data on rice purchases.

To estimate the amount of missing rice, one needs two quantities: the amount of rice that is supposed to arrive in a village each month and the amount of rice actually received by villagers. The difference between these two quantities represents an estimate for how much rice goes missing during the distribution process. However, as noted above, selling rice on the side is not the only way that local officials can skim benefits from the program. They can also sell rice to beneficiaries at a price higher than the official copay price. Thus, I estimate the amount of corruption as the amount of "missing subsidy" in the village, where the value of the intended subsidy for a village is the village's monthly rice quota multiplied by the difference between the local market price for rice and the official copay price for Raskin and the value for the subsidy actually received is the amount of rice actually received by villagers multiplied by the difference between the local market price for rice and the price actually paid by the villagers for Raskin. Thus, I estimate corruption both on the price and on the quantity dimension. In the Appendix, I replicate all the analysis from the paper using just the quantity dimension, and the results are similar.

To estimate the village's intended subsidy, I use administrative data on the village's monthly rice quota (in kg) and household survey data on local market price for rice of similar quality to Raskin. The amount of the village's monthly rice quota is equal to the number of Raskin-

eligible households in a village according to government administrative data times 15 kg, each household's monthly rice allotment. However, this may not equal the amount of rice that the local officials actually obtain from government warehouses. Skimming rice from the program could happen anywhere between government procurement, delivery to warehouses, and delivery from warehouses to villages. Yet, there is reason to believe that a significant portion of the rice goes missing at the village level: Over 70% of the overall variance in missing rice is between villages rather than between sub-districts (where warehouses are located).

To estimate the subsidy received by households within a village, I use household survey data on rice purchases. I utilize data from both survey waves described above, conducted in two separate hamlets within each village, covering a total of 19 households per village. In each survey wave, households were asked about the prior three months of Raskin purchases, including whether they purchased Raskin, the amount purchased, and the copay price. Obtaining purchase data across multiple months is important in obtaining an accurate estimate of the amount of missing rice. Local officials could skim rice either by taking a little off the top each month or by distributing rice in some months but diverting the entire rice quota in other months. For each household, I average rice purchases over the prior three months to obtain an average amount purchased each month. Because households were randomly sampled from the village population, one can use household rice purchases to estimate the total Raskin subsidy received by the village.[20]

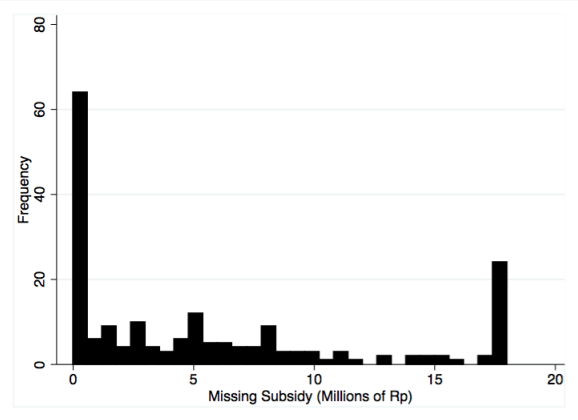
Using this method, I estimate that 26% of the total intended subsidy for the villages in the sample goes missing. This estimate is close to Olken (2006), who estimates that 18% of Raskin rice goes missing. The estimate here is likely higher because it estimates corruption along the price dimension as well as the quantity dimension. Also, similar to Olken (2006), I find that the majority of missing subsidy is concentrated in a few villages. In 29% of villages in the sample, I estimate no missing subsidy and, in an additional 12% of villages, I estimate skimming below 10%. The 10 villages with the most missing subsidy in the sample contain nearly 40% of the total estimated

[20]To estimate village-level program leakages, I use post-stratification weights for each household in the survey sample, where $w_i = {}^{N_p}/_{N_{\hat{p}}}$, where N_p is the number of Raskin-eligible households in the village and $N_{\hat{p}}$ is the number of Raskin-eligible households in the survey sample. Weights for Raskin-ineligible households are calculated similarly.

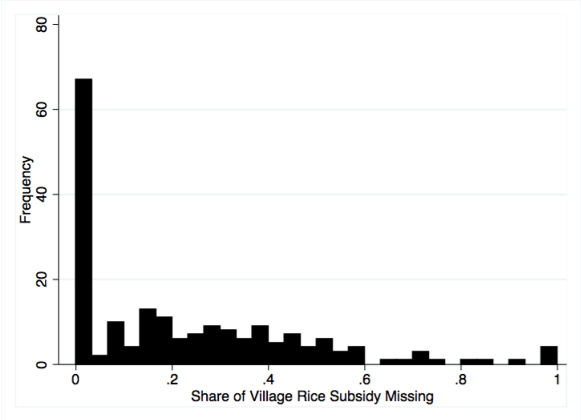
missing subsidy in the sample. Figure 4a shows the distribution of missing subsidy estimates.

Figure 1.4: Distribution of Missing Subsidy

(a) Missing Subsidy (Millions of Rp.)



(b) Share of Village Subsidy Missing



However, estimates of total missing subsidy in the village does not necessarily measure the degree of corruption in a village, since villages with larger populations of eligible households have more subsidy that can potentially go missing. Thus, I measure corruption as the *share of total subsidy that goes missing in a village*. Figure 4b shows the distribution of share of missing subsidy estimates in the sample. On average, villages are missing 23% of the intended subsidy.

Several potential concerns arise with the accuracy of the corruption estimates. First, in some villages, households report purchasing no Raskin at all during certain months, or, for three villages in the sample, during any of the months asked about on the survey. On the one hand, this could represent the maximum amount of corruption, when a local official is diverting 100% of the program's resources away from eligible households. On the other hand, failing to distribute rice at all within a village could occur because of an issue with rice distribution or procurement that had nothing to with actions by local officials. Thus, I also replicate all analysis recoding household Raskin subsidy values from zero to missing if no households in the village reported purchasing any Raskin in that month. This coding decision does not affect results (reported in Appendix Table 3).

Second, some villages exhibit quite high levels of corruption. To ensure that results are not

driven by outliers, I take two precautions. I examine a simple binary indicator of whether any corruption is estimated or not. I also perform a jackknife analysis, dropping one village at a time and reestimating analysis. Third, due to the small number of households sampled per village, there may be a high degree of variability in potential village-level estimates for corruption. I conducted a second jackknifing exercise aimed to account for this variability, described in the Robustness section.

1.5.5 *Controls*

I control, first, for individuals' economic self-interest in fuel subsidies and their economic self-interest in other forms of social assistance. I use three indicators of economic self-interest in fuel subsidies, including: the share of households' monthly consumption that is spent on transportation costs, whether or not the households owns a vehicle (motorboat, car, truck, or motorbike), and whether the household owns a field used for agriculture. Subsidized fuel is an important input in agricultural production, so households that make their livelihoods from agriculture may derive greater benefits from the subsidy. I calculate the share of household income used on transportation rather than the total amount spent on transportation to capture how economically important subsidies are from the perspective of the household balance sheet. Poorer households may consume less fuel than richer households, but fuel subsidies may be more economically important to them if fuel consumption represents a higher share of household income. Finally, I include the distance between the village and the sub-district capital, a measure of village remoteness. More remote villages tend to derive greater value from fuel subsidies as transportation costs represent a higher share of the cost of goods and as it costs more to to distribute fuel to these areas.

I also include three dimensions of household economic status which may make have a greater economic self-interest in transfer programs as compared to fuel subsidies. These include household per capita monthly consumption (logged), whether the head of household is female (a strong predictor of poverty in Indonesia), and whether the household has experienced a death, a major illness, a job loss, or a crop failure in the past 12 months. Table 1 reports household summary statistics for the sample.

Table 1.1: Household Summary Statistics

	Sample	Eligible	Ineligible
Economic Interest in Fuel Subsidies			
- Transportation as a share of monthly consumption	0.055	0.044	0.059
	(0.061)	(0.054)	(0.063)
- Household owns a vehicle?	0.624	0.405	0.708
	(0.484)	(0.491)	(0.455)
- Household owns a field?	0.318	0.211	0.359
	(0.466)	(0.408)	(0.480)
Household Economic Status			
- Household per capita monthly consumption (logged)	13.00	12.75	13.09
	(0.612)	(0.503)	(0.625)
- Female head of household	0.083	0.099	0.077
	(0.275)	(0.298)	(0.266)
- Death in household in past 12 mos.	0.026	0.025	0.026
	(0.159)	(0.158)	(0.159)
- Major illness in household in past 12 mos.	0.158	0.201	0.142
	(0.365)	(0.401)	(0.349)
- Job loss in household in past 12 mos.	0.095	0.113	0.089
	(0.294)	(0.317)	(0.284)
- Crop failure in past 12 mos.	0.178	0.189	0.174
	(0.383)	(0.391)	(0.379)
Observations	1940	1187	753

Standard deviations in parentheses.

I also include several control variables that may be associated with attitudes towards fuel subsidies, including the education of the head of household and a host of village characteristics. These include: village population (logged), the number of schools per capita in the village, the number of religious buildings per capita in the village, whether the village head is elected or appointed, the tenure of the current village head (in months), and ethnic and religious fragmentation in the village. Table 2 reports village-level summary statistics.

Table 1.2: Village Summary Statistics

	Mean	SD	Min	Max
- Distance to sub-district	6.48	9.10	0.20	93.89
- Number of households in village (log)	4.20	0.46	3.09	5.38
- Number of schools per 1,000 households	2.74	1.39	0.00	9.13
- Number of religious buildings per 1,000 households	4.88	3.61	0.37	18.09
- Village Head is elected	0.67	0.47	0	1
- Village Head tenure (months)	36.06	38.02	0	182
- Ethnic fragmentation	0.76	0.30	0.15	1
- Religious fragmentation	0.92	0.12	0.34	1
N	194			

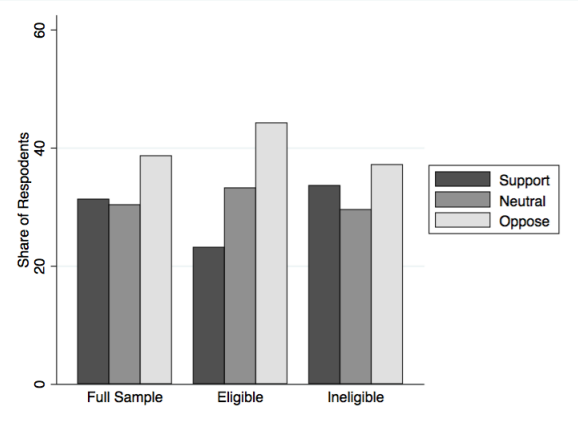
1.6 Results: Observational Analysis

1.6.1 *Levels of Support for Fuel Subsidies*

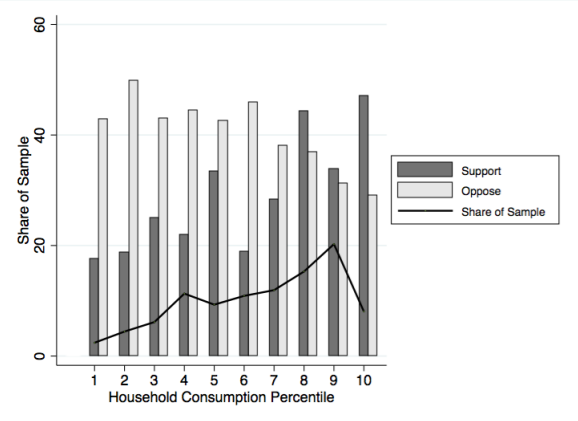
Figure 5a shows the level of support for fuel subsidies among all households in the sample, among households eligible for social programs, and among households ineligible for social programs. Figure 5b reports the level of support for fuel subsidies among all households within each consumption decile. Several facts stand out. First, the distribution of responses shows an overall minority of respondents support increasing government spending on fuel subsidies (28%), and 39% of respondents support decreasing government spending on fuel subsidies. Thus, a greater overall share of respondents select to decrease spending on fuel subsidies than to increase spending on fuel subsidies. This is true for all income groups except for the richest 20% of households (Figure 5b). The poorest households in the sample exhibit the lowest levels of support for the policy: Only 20% of households eligible for social programs select to increase government spending on fuel subsidies, and 45% select to decrease government spending on fuel subsidies. But support among ineligible households is not overwhelming, either. 37% of ineligible households select to decrease spending on fuel subsidies, and 30% select to increase it.

Figure 1.5: Support for government spending across policy domains

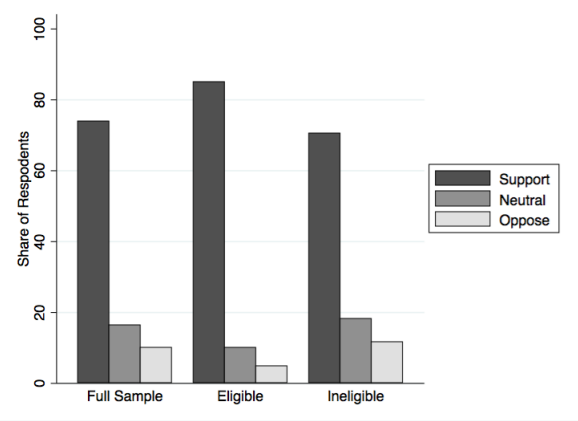
(a) Support for gas subsidies by eligibility status



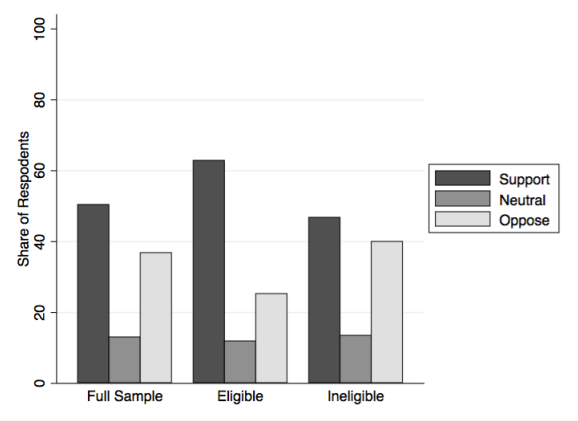
(b) Support for gas subsidies by consumption decile



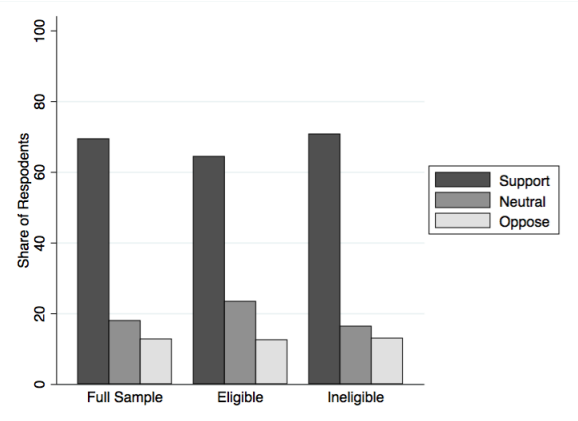
(c) Support for subsidized rice by eligibility status



(d) Support for unconditional cash transfers by eligibility status



(e) Support for health fee waivers by eligibility status



Figures 5a-e shows the share of respondents that select to increase government spending ("support"), decrease government spending ("oppose"), or do not select the specified policy as one of the 3 policies on which to either increase or decrease government spending ("neutral"). Figures 5a, and 5c-e show this by eligibility status, and Figure 5b shows the share of respondents within each consumption decile that select either to increase or decrease government spending on fuel subsidies. The line shows the share of the sample within each consumption decile. Consumption deciles are defined based on World Bank (2012b).

Levels of support for fuel subsidies can be better contextualized when compared to support for targeted transfer programs. Figures 5c, 5d, and 5e show mean levels of support by household eligibility status for Raskin, unconditional cash transfers, and health fee waivers respectively, the three largest forms of targeted social assistance. Support for targeted transfer programs is considerably stronger than support for fuel subsidies: 78% of all households would like to increase government spending on Raskin, 53% of all households would like to increase government spending on unconditional cash transfers, and 69% of all households would like to increase government spending on health fee waivers. Taken together, these summary statistics indicate that fuel subsidies are only moderately popular as compared to targeted transfer programs, and less so among the poor than the non-poor. This runs against the idea that the social contract within oil-producing countries is premised on the receipt of visible and tangible benefits from oil wealth. A substantial portion of citizens support scaling back government spending on fuel subsidies.[21]

1.6.2 *Corruption and Support for Fuel Subsidies*

How does corruption affect attitudes towards fuel subsidies? Table 3 examines the relationship between corruption in the delivery of the Raskin program and support for gasoline subsidies for those households eligible to purchase Raskin.[22] The relationship between local corruption and attitudes towards gasoline subsidies is highly significant. This is true whether corruption is measured as the share of overall subsidy that goes missing, a binary indicator that any subsidy goes missing, the amount of the overall subsidy that goes missing, or the amount of the overall subsidy that goes missing with values top-coded at the 90th percentile to account for the influence of outliers.

Figure 6 illustrates the magnitude of this effect. When corruption levels are near zero, poor households are more than three times more likely to oppose rather than to support gasoline subsidies. As the share of missing subsidies approach 100%, households become more likely to support rather than to oppose gasoline subsidies, although, due to the relatively small number of villages

[21] Ideally, to derive the most accurate picture of patterns of support, I would have randomized the ordering of the policies on the list. However, this was not logistically possible for this survey. Thus, it is possible that the ordering on the list affected the response rates to certain policies.

[22] These are the households affected by corruption in the delivery of targeted social assistance programs.

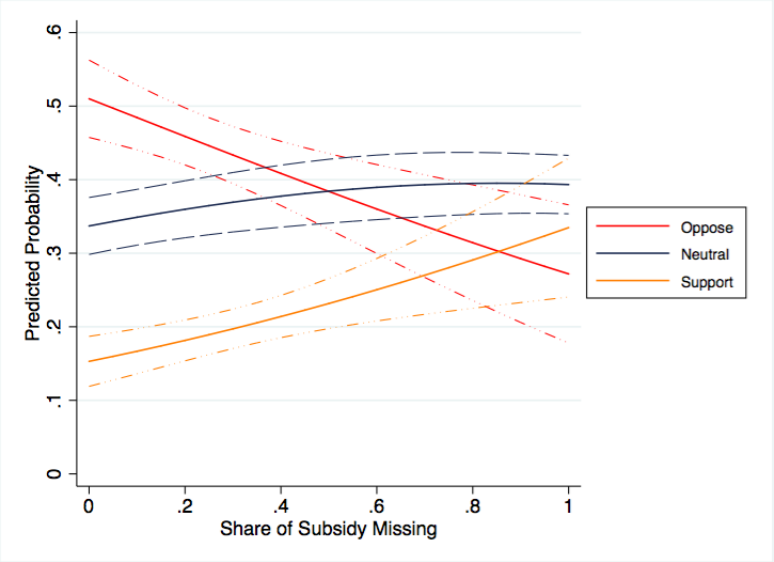
with such high levels of corruption, the confidence intervals are wide.

Table 3: Corruption and Support for Fuel Subsidies

	(1)	(2)	(3)	(4)
Share of subsidy missing (%)	1.02 (0.36)***			
Any subsidy missing (dummy)		0.51 (0.19)***		
Amt of subsidy missing ('000s Rp)			38.7 (12.9)***	
Amt of subsidy missing, top-coded ('000s Rp)				12.3 (3.35)***
Vehicle ownership	0.21 (0.16)	0.22 (0.16)	0.21 (0.16)	0.23 (0.16)
Transportation as a share of consumption	1.41 (1.09)	1.45 (1.08)	1.44 (1.04)	1.26 (1.04)
Household has agricultural field	0.05 (0.16)	0.06 (0.16)	0.05 (0.15)	0.04 (0.15)
Log per capita consumption	0.34 (0.15)**	0.34 (0.15)**	0.37 (0.15)**	0.39 (0.15)***
Female-headed household	-0.03 (0.21)	0.04 (0.22)	-0.01 (0.22)	-0.15 (0.20)
Death in household	0.19 (0.41)	0.17 (0.40)	0.21 (0.40)	0.24 (0.41)
Major illness in household	-0.23 (0.17)	-0.21 (0.17)	-0.25 (0.17)	-0.28 (0.17)
Job loss in household	-0.70 (0.27)***	-0.74 (0.27)***	-0.66 (0.27)**	-0.67 (0.27)**
Crop failure in household	-0.25 (0.19)	-0.26 (0.19)	-0.26 (0.19)	-0.25 (0.19)
Head of HH education	0.08 (0.07)	0.08 (0.07)	0.09 (0.07)	0.08 (0.07)
Village population, # HH (logged)	-0.11 (0.15)	-0.08 (0.16)	-0.20 (0.16)	-0.24 (0.16)
Distance to sub-district capital (km)	0.02 (0.005)***	0.01 (0.004)***	0.01 (0.005)***	0.02 (0.005)***
Schools per 1,000 HH	0.09 (0.06)	0.09 (0.06)	0.10 (0.06)*	0.09 (0.06)
Religious buildings per 1,000 HH	0.005 (0.03)	0.007 (0.03)	0.006 (0.03)	-0.003 (0.03)
Village head is elected	0.08 (0.25)	0.14 (0.24)	0.10 (0.24)	0.13 (0.25)
Village head tenure (in months)	-0.002 (0.002)	-0.002 (0.002)	-0.001 (0.002)	-0.002 (0.002)
Ethnic fragmentation	0.33 (0.42)	0.11 (0.38)	0.32 (0.41)	0.40 (0.42)
Religious fragmentation	-2.12 (0.74)***	-1.91 (0.71)***	-1.99 (0.71)***	-2.39 (0.77)***
N	1,186	1,186	1,186	1,186

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Standard errors clustered by village in parentheses.*

Figure 1.6: Impact of Corruption on Predicted Probability of Support for Fuel Subsidies



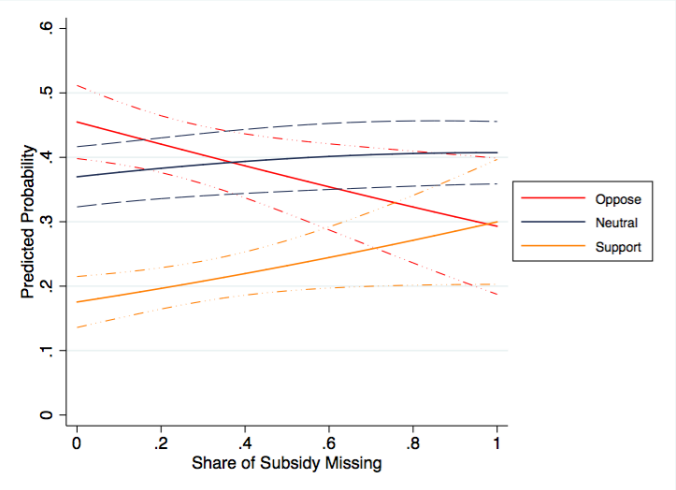
Note: Figure 4 plots the predicted probabilities from an ordered logit models using a 3-category outcome variable where "-1" = decrease government spending on fuel subsidies ("oppose"), "0" = selected neither to increase nor decrease spending on fuel subsidies ("neutral"), and "1" = increase government spending on fuel subsidies ("support"). Standard errors are clustered by village. Dotted lines are 90% confidence intervals. Predicted probabilities are estimated based on Model 1 in Table 3. All other variables are held at their means.

Corruption matters on the extensive margin as well: An individual living in a village with corrupt local officials is 13 percentage points more likely to support increasing spending on fuel subsidies, a 62% increase from the sample mean for eligible households in villages with no corruption (calculated based on Table 3, Model 2). The relationship between corruption and attitudes towards fuel subsidies is robust to a number of alternative specifications, including the inclusion of district fixed effects (Table 5, Appendix) and dropping observations for months in which no one in the village reports purchasing any rice (Table 6, Appendix). Table 7 (Appendix) replicates the analysis looking only at the quantity dimension of corruption, and the substantive results are consistent.

Further, one may be concerned that because local officials exert high influence over the distribution of social programs within villages, a household's eligibility status may not be a perfect indicator for whether or not the household is affected by corruption. This is because local leaders may use their discretion to redistribute program benefits to poor households within the village that

are erroneously left off of targeting lists. If village leaders use their discretion over program implementation to both distribute program benefits more widely than intended by government targeting lists and to skim program benefits, then corruption may affect a wider population than just eligible households. Thus, Table 8 (Appendix) and Figure 7 replicate analysis by testing the hypothesis on all households in the sample below the median sample household consumption level rather than just on households eligible for Raskin. Substantive results are similar: for poorer households, as corruption rises, so does support for gasoline subsidies.

Figure 1.7: Impact of Corruption on Predicted Probability of Support for Fuel Subsidies, for Households Below Median Consumption Levels



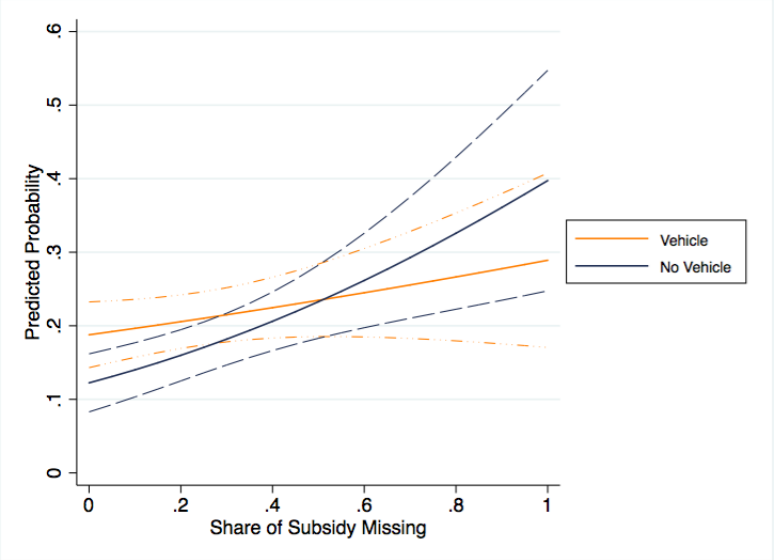
Note: Figure 7 plots the predicted probabilities from an ordered logit models using a 3-category outcome variable where "-1" = decrease government spending on fuel subsidies ("oppose"), "0" = selected neither to increase nor decrease spending on fuel subsidies ("neutral"), and "1" = increase government spending on fuel subsidies ("support"). Standard errors are clustered by village. Dotted lines are 90% confidence intervals. Predicted probabilities are estimated based on Model 1 in Table 3. All other variables are held at their means. Only households below median consumption levels for the sample are included in the analysis.

Figure 8 probes this relationship further by attempting to distinguish between the two mechanisms proposed in the theory section for how corruption could influence support for fuel subsidies. The first is simply that corruption could shift the economic value of fuel subsidies above the economic value of targeted assistance programs, while the second is that individuals with corrupt local institutions prefer fuel subsidies because they lack trust in the local politicians and bureaucrats responsible for implementing targeted social assistance programs. It is also possible that corruption

affects attitudes through both mechanisms.

I investigate this by examining whether corruption induces support for fuel subsidies even among those households that are eligible for social programs but do not own vehicles. In other words, does corruption influence only those individuals that could benefit the most economically from fuel subsidies? If corruption were only working through this economic mechanism, then we would expect corruption to primarily influence individuals with vehicles. However households without vehicles are just as affected by corruption. This provides some evidence that corruption affects attitudes in ways beyond simply altering the relative economic benefits of various policy domains.

Figure 1.8: Impact of Corruption on Predicted Probability of Support for Fuel Subsidies, by Vehicle Ownership



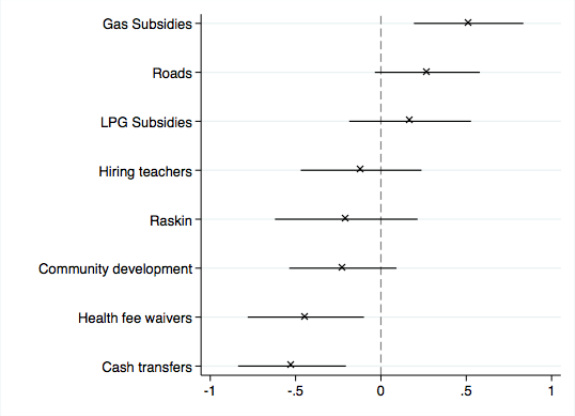
Note: Figure 8 plots the predicted probabilities from an ordered logit model of selecting to increase spending on fuel subsidies using the 3-category outcome variable, where "-1" = decrease government spending on fuel subsidies ("oppose"), "0" = selected neither to increase nor decrease spending on fuel subsidies ("neutral"), and "1" = increase government spending on fuel subsidies ("support"). Dotted lines are 90% confidence intervals. The blue line plots the predicted probabilities from a model on households eligible for social programs that do not own vehicles, and the orange line plots the predicted probability for a separate model on households eligible for social programs that own vehicles. All other variables are held at their means.

1.6.3 *Extending the Argument: Corruption and Support for Other Programs*

If this argument presented in this paper is correct, and the primary reason that corruption shapes

attitudes towards fuel subsidies is that subsidies are less vulnerable to local corruption, then corruption should also affect attitudes towards many forms of government spending, depending on how vulnerable each spending category is to local corruption. Thus, I examine the effect of corruption on support for each category of government spending listed in the survey questions that form the dependent variable. Figure 9 assesses the relationship between the binary measure of corruption ("any subsidy missing") and support for different forms of government spending. Figure 9 plots the coefficients and standard errors from eight separate ordered logit models. The dependent variable in each model is the three-category outcome variable indicating whether the respondent chose to increase, neither to increase nor decrease, or to decrease spending on a given policy area. All models include the same controls as reported in Table 3.

Figure 1.9: Effect of Corruption on Attitudes Towards Government Spending



Note: Figure 9 plots the coefficients and standard errors for "any subsidy missing" in 8 separate models. In each model, the dependent variable is a 3-category outcome variable ("-1" = decrease government spending on "x" policy, "0" = selected neither to increase nor decrease spending on "x" policy, and "1" = increase government spending on "x" policy). All models control for vehicle ownership, transportation as a share of consumption, field ownership, household per capita consumption (logged), female-headed household, death in household, major illness in household, job loss in household, crop failure, head of household education, village population (logged), the distance between the village and the sub-district capital, the number of schools per 1,000 households in the village, the number of religious buildings per 1,000 households in the village, whether the village government head is elected or appointed, the tenure of the current village head (in months), and ethnic and religious fragmentation in the village. Standard errors are clustered by village. The dotted reference line indicates statistical significance at the 90% level.

The theory predicts attitudes across a broad range of government spending areas. Corruption tends to increase support for expanding government spending on programs that are less vulnerable

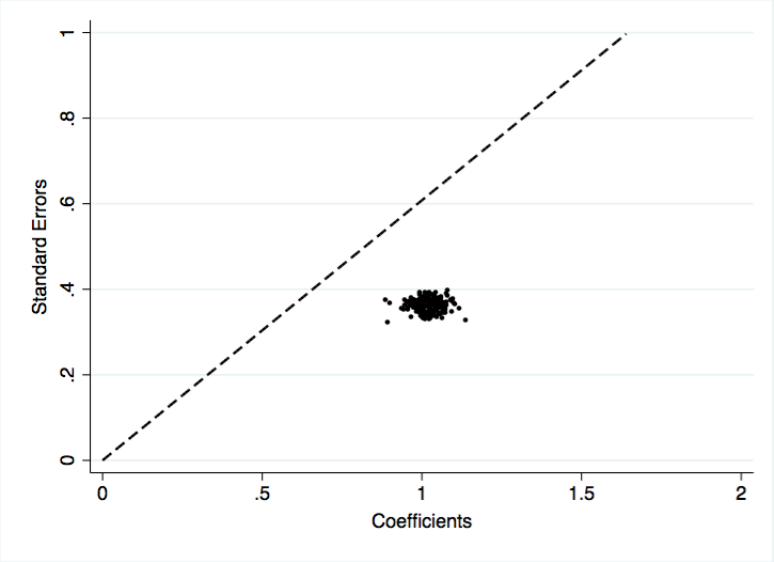
to local corruption, like gasoline subsidies, building roads, and LPG subsidies. By contrast, corruption tends to diminish support for expanding government spending on programs that are more vulnerable to local corruption, like Raskin, health fee waivers for the poor[23], and cash transfers to the poor.

1.6.4 *Robustness*

To ensure that results are not driven by outliers, I take two precautions. First, I perform a jackknife analysis, dropping one village at a time and reestimating the main model 194 times (Table 3, Model 1). Figure 10 plots the coefficients and standard errors from this exercise. Coefficients are similar, and all are statistically significant.

[23] Although there are not rents to capture in the implementation of health fee waivers in the same way that local officials can capture rents from the distribution of in-kind food aid and cash transfers, local officials still exert a high degree of discretion in deciding who within the village will have access to the program.

Figure 1.10: Jackknife Estimates of Effect of Corruption on Support for Gasoline Subsidies

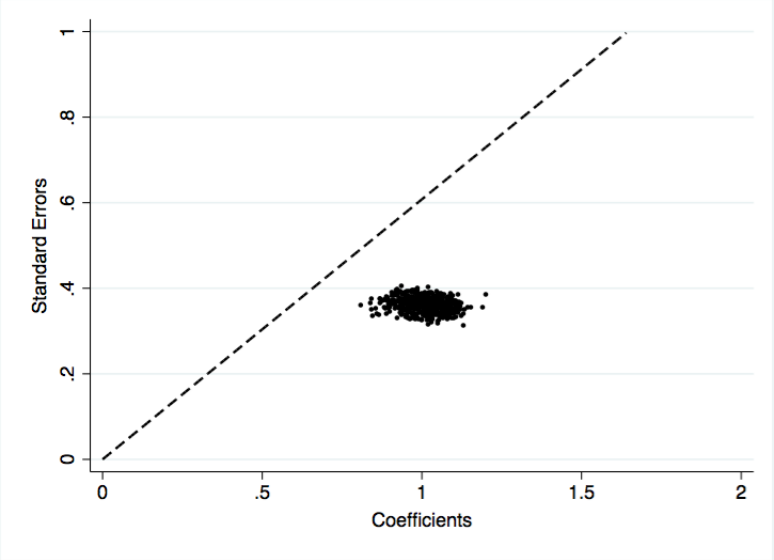


Note: Figure 10 plots the coefficients and standard errors for share of subsidy missing in 194 separate models. Each model drops one village. Each model is an ordered logit model for share of subsidy missing on a 3-category outcome variable ("-1" = decrease government spending on fuel subsidies, "0" = selected neither to increase nor decrease spending on fuel subsidies, and "1" = increase government spending on fuel subsidies). All models control for vehicle ownership, transportation as a share of consumption, field ownership, household per capita consumption (logged), female-headed household, death in household, major illness in household, job loss in household, crop failure, head of household education, village population (logged), the distance between the village and the sub-district capital, the number of schools per 1,000 households in the village, the number of religious buildings per 1,000 households in the village, whether the village government head is elected or appointed, the tenure of the current village head (in months), and ethnic and religious fragmentation in the village. Standard errors are clustered by village. The dotted reference line indicates statistical significance at the 90% level.

Second, due to the small number of households sampled per village, there may be a high degree of variability in potential village-level estimates for corruption. To account for this variability, I conduct an exercise which asks the hypothetical question: what would the estimate of corruption be for this village if this household had not been selected into the sample? Dropping one household at a time and reestimating corruption values results in 19 different potential values for corruption for each village. I then randomly select one of these 19 values for each village and reestimate all analyses 500 times. If results are similar across models, this lends confidence to the overall relationship between corruption and attitudes and mitigates concerns that findings are driven by outliers and variability due to sampling a relatively small number of respondents per village. Figure 11 plots the coefficients and standard errors from this exercise. Although the coefficient estimates

are noisy, the estimates are consistently positive and significant even accounting for the sampling variability. Table 9 (Appendix) replicates the main analysis using the maximum and minimum possible corruption values for each village, and the main relationship holds.

Figure 1.11: Coefficients and Standard Errors with Sampling Variability



Note: Figure 8 plots the coefficients and standard errors for share of subsidy missing for 500 separate models. The dotted reference line indicates statistical significance at the 90% level. Each point represents the coefficient and standard error for estimated missing rice in a village calculated from a random draw for each village of one of the 19 potential values for missing rice in each village. I first calculate the 19 potential estimates for missing rice in a village by randomly dropping one survey respondent per village and reestimating missing rice, imagining that respondent hadn't been selected into the sample. Second, I randomly draw one of the 19 estimates for missing rice for each village and reestimate the ordered logit model in Model 1, Table 3. I do this 500 times. Each point on the graph represents the coefficient and standard errors on missing rice for 500 draws.

Together, these exercises lend evidence that the estimated relationship between corruption and attitudes is not drive by outliers or by imprecise corruption estimates.

1.7 Matching to Improve Inference

A problem with the empirical analysis in this study, as in others that assess the effects of corruption, is that corrupt villages may be different from uncorrupt villages along other dimensions. For example, villages in Indonesia vary in the extent to which they are ethnically heterogeneous. Ethnically diverse villages may be less capable of collective action to monitor local officials, enabling

higher levels of corruption. Olken (2006) finds evidence that more diverse villages are more corrupt. And, ethnic diversity could independently affect attitudes towards government spending, say, by affecting individuals' willingness to redistribute wealth. When differences between treatment and control units are of concern, matching can be used to balance the distribution of covariates to help reduce bias in the estimates.

The objective of matching is to compare cases in which all other causal variables are as similar as possible (except for corruption) so that differences in the outcome can be attributed to the treatment (corruption). The original data contains 138 villages that experience corruption in the implementation of transfer programs, compared to 56 villages with no estimated corruption. I use propensity score matching to construct the matched sample. To estimate propensity scores, I run a logistic regression with all village-level covariates as predictors. Two treatment villages that lie outside the common support of the estimated propensity score are discarded.[24] I then conduct one-to-one nearest neighbor matching[25] based on the estimated propensity score, allowing for replacement of control villages.[26] The matching performs well, with 79% of the matches having an absolute propensity score difference of less than 0.01 and all having a difference of less than 0.03 (the caliper used). Preprocessing the data in this way discards 16 control villages. The matched data set then contains 176 villages (136 treatment villages and 40 control villages).

Matching leads to a balance improvement for most covariates (see Table 13, Appendix). An exception is that matching actually reduces balance in whether village heads are elected or appointed, a key difference in local institutions in Indonesia (see Martinez-Bravo 2014). Although whether village heads are elected or appointed is not significantly correlated with corruption in the sample, democratic institutions are theoretically linked to both corruption and political attitudes, making balance along this dimension important. Interestingly, the raw data exhibits almost no bias on this dimension. I thus conduct an additional matching exercise which forces balance on whether

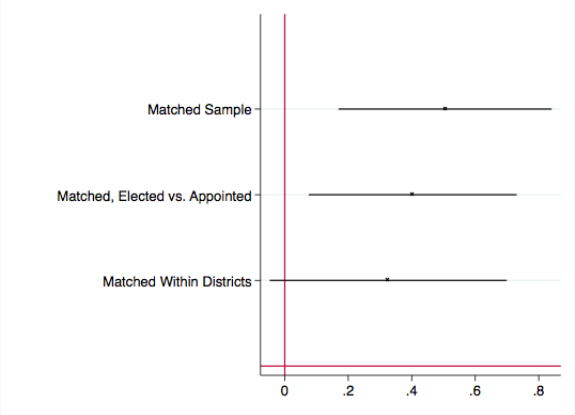
[24] I use a caliper of 0.03 to ensure that poor matches are not included. Using a caliper of 0.02 would mean that 12 treatment villages are off the common support. Results are robust to using the smaller caliper (available upon request).

[25] Results are robust to 2:1 and 3:1 matching as well (available upon request). I show the results from the 1:1 matching since not many observations are discarded.

[26] This allows a given "control" village to match to more than one "treatment" village, useful when the number of control units is fewer than the number of treatment units (Ho et al. 2007).

village heads are elected vs. appointed (see Table 14, Appendix). The tradeoff with this exercise is that additional villages are discarded from the sample, resulting in a sample of 160 villages. Finally, to reduce concerns that unobservables that are spatially correlated cause both corruption and political attitudes, I match within districts. The tradeoff with forcing matches within districts is that additional villages are discarded from the sample, resulting in a sample of 125 villages, and the quality of the matches along observables is not as high.[27] I show the results from all three matching exercises.

Figure 1.12: Coefficients and Standard Errors with Sampling Variability



Note: Figure 12 plots the coefficients and standard errors for whether any subsidy is missing (any corruption) for three separate ordered logit models. All models control for vehicle ownership, transportation as a share of consumption, field ownership, household per capita consumption (logged), female-headed household, death in household, major illness in household, job loss in household, crop failure, head of household education, village population (logged), the distance between the village and the sub-district capital, the number of schools per 1,000 households in the village, the number of religious buildings per 1,000 households in the village, whether the village government head is elected or appointed, the tenure of the current village head (in months), and ethnic and religious fragmentation in the village. Standard errors are clustered by village. The reference line indicates statistical significance at the 90% level.

Following Ho et al. (2007), I then estimate a parametric model using only the data in the matched sample.[28] I run the same model as reported in Table 3, incorporating the weights from the matching. The coefficients and standard errors for the effect of corruption from each of the three matching exercises are plotted in Figure 12. Across the three matching exercises, corruption

[27]The average difference between propensity scores doubles (although still small in absolute value, from 0.006 to 0.012).

[28]Using the preprocessed data (which compares only like units) ensures that results are not very sensitive to model specification (Ho et al. 2007).

increases support for gasoline subsidies.[29] Relying on the first matching exercise, an individual living in a village with corrupt local officials is 12.5 percentage points less likely to support decreasing spending on fuel subsidies, a 28% decrease from the sample mean. Corruption increases the likelihood that an individual supports increasing spending on fuel subsidies by 68% compared to the sample mean. The substantive effects from the matching exercise are almost identical to those reported in the observational analysis. If anything, the effect of corruption is a little larger using the preprocessed data.

1.8 Conclusions

Using household survey data from Indonesia, this paper tests how variation in local institutional performance in the delivery of targeted social assistance programs shapes citizens' support for fuel subsidies. This paper measures local institutional performance by estimating corruption in the delivery of Indonesia's largest targeted social assistance program, Raskin, using a "gap" measurement method. Higher levels of corruption are associated with increased support for fuel subsidies. I argue that this is because whereas targeted social assistance programs require the involvement of potentially corruption local officials, individuals can access fuel subsidies without going through bureaucratic channels. Thus, when their local officials are corrupt, citizens will prefer for the government to channel economic benefits to the population via fuel subsidies, bypassing local institutions. I increase confidence in the finding through a host of robustness checks and through matching methods.

This finding has important implications for current policy discussions on fuel subsidy reforms. In its recent guide on how to successfully implement energy-pricing reforms, the IMF (2013) argues that compensating the poor for increases in fuel prices through improved social policy is key to successful reform. However, many countries utilize fuel subsidies as a redistributive tool precisely because they lack the institutional capacity to execute alternative forms of social policy (Victor 2009). My analysis suggests that institutional weakness in implementing social policy

[29]The coefficient on corruption is not statistically significant at conventional levels for the sample where matching is forced within districts (p-value=0.15). However, the sample size is also smaller (N=780).

is an important cause of public support for fuel subsidies among the poor. Initiatives to replace fuel subsidies with social policy are unlikely to receive much support while institutions are weak. Thus, strengthening institutional capacity to deliver alternative forms of social assistance for the poor may be an important prerequisite for fuel subsidy reforms.

The paper also has several broader implications. First, the results show that energy and environmental policy cannot be studied in isolation from other governmental initiatives to provide income support and relief from market risks. Because energy consumption represents a significant share of households' overall consumption, any governmental interventions in energy pricing affect household consumption patterns. Removing government price ceilings on energy also subjects households to the substantial price fluctuations in international commodity markets. Thus, the potential effects of changes in energy pricing must take into account the other ways that governments intervene in the economy that affect household income and risk exposure. Second, the findings illustrate that local policy implementation can significantly shape support for national policies. The way that individuals experience social policies may vary significantly depending on how local politicians and bureaucrats implement those policies, even in developed countries. This is an important and understudied dimension of understanding individuals' attitudes towards redistribution and social policy generally.

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1.10 Appendix Tables and Figures

Table 1.4: Sample Demographics

		Raw			Weighted		
	Population	Sample	Eligible	Ineligible	Sample	Eligible	Ineligible
Overall							
- % hh in urban areas	48.3	30.6	30.6	30.7	48.1	48.5	48.0
- % hh eligible for Raskin	27.0	61.2	100.0	0.0	27.6	100.0	0.0
- % hh in bottom 40%	40.0	31.1	37.0	22.0	39.9	54.1	34.5
- % hh in bottom 10%	10.0	4.5	5.7	2.7	10.0	17.8	7.0
Urban							
- % hh in bottom 40%	32.5	23.1	26.7	17.3	32.5	46.4	27.2
- % hh in bottom 10%	7.4	2.0	3.0	0.4	7.4	18.5	3.2
Rural							
- % hh in bottom 40%	47.1	34.8	41.5	24.1	46.8	61.4	41.3
- % hh in bottom 10%	12.5	5.6	6.9	3.6	12.3	17.0	10.5

Note: Table 4 provides population demographics, raw summary statistics from the sample, and weighted summary statistics using entropy balancing. Population demographic statistics come from the World Bank (2012) and reflect population demographics from 2010. I count households in the bottom 40% (10%) if their reported monthly per capita expenditures are closer to the average per capita monthly expenditures for the bottom 40% (10%) than to the bottom 50% (20%) by province. Note that average by-province monthly per capita expenditure by consumption decile are from 2010.

Table 1.5: Corruption and Support for Fuel Subsidies, with District Fixed Effects

	(1)	(2)	(3)	(4)
Share of subsidy missing (%)	1.04***			
	(0.38)			
Any subsidy missing (dummy)		0.49**		
		(0.20)		
Amt of subsidy missing (M Rp)			0.04***	
			(0.01)	
Amt of subsidy missing, top-coded (M Rp)				0.01***
				(0.00)
N	1,186	1,186	1,186	1,186
INCLUDES CONTROLS	YES	YES	YES	YES
INCLUDES DISTRICT FE	YES	YES	YES	YES

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Standard errors in parentheses. Coefficients and standard errors come from ordered logit models using a 3-category outcome variable ("-1" = decrease government spending on fuel subsidies, "0" = selected neither to increase nor decrease spending on fuel subsidies, and "1" = increase government spending on fuel subsidies). Standard errors are clustered by village. All models contain household- and village-level control variables as in Table 3 and district fixed effects.*

Table 1.6: Corruption and Support for Fuel Subsidies, Recoding Months with No Rice Purchases as Missing

	(1)	(2)	(3)	(4)
Share of subsidy missing (%)	0.99***			
	(0.36)			
Any subsidy missing (dummy)		0.39**		
		(0.18)		
Amt of subsidy missing (M Rp)			0.04***	
			(0.01)	
Amt of subsidy missing, top-coded (M Rp)				0.01***
				(0.00)
N	1,186	1,186	1,186	1,186
INCLUDES CONTROLS	YES	YES	YES	YES

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Standard errors in parentheses. Coefficients and standard errors come from ordered logit models using a 3-category outcome variable ("-1" = decrease government spending on fuel subsidies, "0" = selected neither to increase nor decrease spending on fuel subsidies, and "1" = increase government spending on fuel subsidies). Standard errors are clustered by village. All models contain household- and village-level control variables as in Table 3. Months with no reported Raskin purchases for the entire village are recoded as missing in all corruption estimates.*

Table 1.7: Corruption and Support for Fuel Subsidies, Measuring Corruption Along Quantity Dimension Only

	(1)	(2)	(3)	(4)
Share of rice missing (%)	1.05***			
	(0.37)			
Any rice missing (dummy)		0.43**		
		(0.19)		
Amt of rice missing ('000s of kg)			0.30***	
			(0.10)	
Amt of rice missing, top-coded ('000s of kg)				0.08***
				(0.02)
N	1,186	1,186	1,186	1,186
INCLUDES CONTROLS	YES	YES	YES	YES

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Standard errors in parentheses. Coefficients and standard errors come from ordered logit models using a 3-category outcome variable ("-1" = decrease government spending on fuel subsidies, "0" = selected neither to increase nor decrease spending on fuel subsidies, and "1" = increase government spending on fuel subsidies). Standard errors are clustered by village. All models contain household- and village-level control variables as in Table 3.*

Table 1.8: Corruption and Support for Fuel Subsidies, Among Below Median Consumption HH

	(1)	(2)	(3)	(4)
Share of subsidy missing (%)	0.70*			
	(0.38)			
Any subsidy missing (dummy)		0.41*		
		(0.23)		
Amt of subsidy missing (M Rp)			0.02	
			(0.01)	
Amt of subsidy missing, top-coded (M Rp)				0.01
				(0.00)
N	992	992	992	992
INCLUDES CONTROLS	YES	YES	YES	YES

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Standard errors in parentheses. Coefficients and standard errors come from ordered logit models using a 3-category outcome variable ("-1" = decrease government spending on fuel subsidies, "0" = selected neither to increase nor decrease spending on fuel subsidies, and "1" = increase government spending on fuel subsidies). Standard errors are clustered by village. All models contain household- and village-level control variables as in Table 3. The sample includes all households under the median consumption level for the sample, regardless of whether they are officially eligible or ineligible to purchase Raskin.*

Table 1.9: Corruption and Support for Fuel Subsidies, Min-Max Corruption Estimates

	(1)	(2)	(3)
Share of subsidy missing, min (%)	0.99***		
	(0.37)		
Share of subsidy missing, mean (%)		1.04***	
		(0.36)	
Share of subsidy missing, max (%)			1.07***
			(0.35)
N	1,186	1,186	1,186
INCLUDES CONTROLS	YES	YES	YES

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Standard errors in parentheses. Coefficients and standard errors come from ordered logit models using a 3-category outcome variable ("-1" = decrease government spending on fuel subsidies, "0" = selected neither to increase nor decrease spending on fuel subsidies, and "1" = increase government spending on fuel subsidies). Standard errors are clustered by village. All models contain household- and village-level control variables as in Table 3. Model 1 estimates the relationship between corruption and support for gasoline subsidies using the minimum possible value for corruption using the household-level jackknifing exercise. Model 2 estimates the relationship between corruption and support for gasoline subsidies using the mean value for corruption using the household-level jackknifing exercise. Model 3 estimates the relationship between corruption and support for gasoline subsidies using the maximum possible value for corruption using the household-level jackknifing exercise.*

Table 1.10: Logit Models for Selecting to Increase Spending on Fuel Subsidies

	(1)	(2)	(3)	(4)
Share of subsidy missing (%)	0.57			
	(0.45)			
Any subsidy missing (dummy)		0.37		
		(0.22)		
Amt of subsidy missing (M Rp)			0.03**	
			(0.02)	
Amt of subsidy missing, top-coded (M Rp)				0.01**
				(0.005)
N	1,186	1,186	1,186	1,186
INCLUDES CONTROLS	YES	YES	YES	YES

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Standard errors in parentheses. Coefficients and standard errors come from logit models using a 2-category outcome variable ("1" = increase government spending on gasoline subsidies, "0" = did not select to increase spending on gasoline subsidies). Standard errors are clustered by village. All models contain household- and village-level control variables as in Table 3.*

Table 1.11: Logit Models for Selecting to Decrease Spending on Fuel Subsidies

	(1)	(2)	(3)	(4)
Share of subsidy missing (%)	-1.33***			
	(0.45)			
Any subsidy missing (dummy)		-0.57***		
		(0.22)		
Amt of subsidy missing (M Rp)			-0.04***	
			(0.02)	
Amt of subsidy missing, top-coded (M Rp)				-0.01***
				(0.005)
N	1,186	1,186	1,186	1,186
INCLUDES CONTROLS	YES	YES	YES	YES

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Standard errors in parentheses. Coefficients and standard errors come from logit models using a 2-category outcome variable ("1" = decrease government spending on gasoline subsidies, "0" = did not select to decrease spending on gasoline subsidies). Standard errors are clustered by village. All models contain household- and village-level control variables as in Table 3.*

Table 1.12: Correlates of Corruption, Village-Level

	(1)
Village population, # HH (logged)	-0.958**
	(0.295)
Distance to sub-district capital (log)	0.257
	(0.214)
Religious buildings per 1,000 HH	-0.079**
	(0.139)
Schools per 1,000 HH	-0.104
	(0.048)
Village Head is elected	-0.371
	(0.499)
Ethnic fragmentation	-0.904
	(0.915)
Religious fragmentation	1.081
	(1.810)
N	194

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Standard errors in parentheses. Coefficients and standard errors come from logit models using a 2-category outcome variable ("1" = village estimated to have corruption, "0" = no estimated corruption).*

Table 1.13: Differences in Means between Corrupt and Un-corrupt Villages Before and After Matching

		Mean		Std. % Bias	% Improvement
		Corruption	No Corruption		
Village population, # HH (logged)	Unmatched	6.84	7.20	-49.9	
	Matched	6.84	6.66	26.6	46.6
Distance to sub-district capital (logged)	Unmatched	1.51	1.30	23.6	
	Matched	1.49	1.21	31.5	-33.4
Religious buildings per 1,000 HH	Unmatched	4.54	5.71	-31.2	
	Matched	4.54	5.49	-22.2	28.7
Schools per 1,000 HH	Unmatched	2.70	2.82	-8.1	
	Matched	2.71	2.73	-1.5	81.7
Village head is elected	Unmatched	0.66	0.70	-7.9	
	Matched	0.66	0.74	-15.7	-98.7
Ethnic fragmentation	Unmatched	0.75	0.79	-15.3	
	Matched	0.75	0.77	-5.8	62.3
Religious fragmentation	Unmatched	0.93	0.90	20.3	
	Matched	0.93	0.95	-14.5	28.5

Table 13 shows shows the means in the village-level covariates before and after matching as well as the percent balance improvement over the raw data. Matching improves balance on most dimensions, except for whether or not village heads are elected and the distance to the sub-district capital.

Table 1.14: Differences in Means between Corrupt and Un-corrupt Villages Before and After Matching, Forcing Balance on Village Elections

		Mean		Std. % Bias	% Improvement
		Corruption	No Corruption		
Village population, # HH (logged)	Unmatched	6.84	7.20	-49.9	
	Matched	6.85	6.77	10.6	78.7
Distance to sub-district capital (logged)	Unmatched	1.51	1.30	23.6	
	Matched	1.51	1.32	21.6	8.5
Religious buildings per 1,000 HH	Unmatched	4.54	5.71	-31.2	
	Matched	4.68	5.24	-14.8	52.6
Schools per 1,000 HH	Unmatched	2.70	2.82	-8.1	
	Matched	2.88	2.64	17.0	-109.9
Village head is elected	Unmatched	0.66	0.70	-7.9	
	Matched	0.75	0.75	0	100.0
Ethnic fragmentation	Unmatched	0.75	0.79	-15.3	
	Matched	0.79	0.78	5.2	65.9
Religious fragmentation	Unmatched	0.93	0.90	20.3	
	Matched	0.93	0.94	-7.1	64.9

Table 14 shows shows the means in the village-level covariates before and after matching as well as the percent balance improvement over the raw data. To force balance in whether or not village heads are elected, I match within elected vs. appointed villages. Matching improves balance on most dimensions, except for the number of schools per capita.

Table 1.15: Differences in Means between Corrupt and Un-corrupt Villages Before and After Matching Within Districts

		Mean		Std. % Bias	% Improvement
		Corruption	No Corruption		
Village population, # HH (logged)	Unmatched	6.84	7.20	-49.9	
	Matched	6.83	6.75	10.6	78.7
Distance to sub-district capital (logged)	Unmatched	1.51	1.30	23.6	
	Matched	1.57	1.39	21.0	11.0
Religious buildings per 1,000 HH	Unmatched	4.54	5.71	-31.2	
	Matched	5.14	5.21	-1.9	93.7
Schools per 1,000 HH	Unmatched	2.70	2.82	-8.1	
	Matched	2.96	3.20	-16.5	-104.1
Village head is elected	Unmatched	0.66	0.70	-7.9	
	Matched	0.77	0.86	-18.9	-140.2
Ethnic fragmentation	Unmatched	0.75	0.79	-15.3	
	Matched	0.81	0.79	6.7	56.1
Religious fragmentation	Unmatched	0.93	0.90	20.3	
	Matched	0.93	0.95	-13.6	33.0

Table 15 shows shows the means in the village-level covariates before and after matching as well as the percent balance improvement over the raw data. Matching improves balance on most dimensions, except for whether or not village heads are elected and the number of schools per capita.

Chapter 2

Risks and Rewards: How Political Institutions Shape Oil Income

2.1 Introduction

Why are some countries flush with cash from oil? The most common answers to this question are geology and oil prices. Countries that combine favorable geology with high oil prices will experience wealth windfalls, with oil revenues falling like "manna from the heavens" into government coffers. In other words, random chance shapes the distribution of oil wealth across countries and determines which governments are fortunate enough to receive revenues that are, in theory, easier to collect and conceal than ordinary tax revenues.

Recent literature has begun to question this assumption, recognizing that politicians can enact policies that increase or decrease the state's oil income. For example, governments can deliberately court investment in the oil sector, seeking to launch discovery efforts and to increase oil production (Menaldo, forthcoming). Policy choices about the taxes and royalties that are assessed on oil production determine the division of oil rents–the surplus of oil revenues after subtracting costs of extraction–between government coffers and private oil companies. These choices can be heavily influenced by politics, as political competition can shape whether or not politicians seek to maximize or reduce the state's claim to rents (Dunning 2010). Politicians also design the structure of the relationship between the government, nationally-owned oil companies (NOCs), and international oil companies (IOCs), determining whether, for example, oil endowments are state-owned and whether NOCs will participate in oil development (Jones Luong and Weinthal 2006, 2010).

In line with these contributions, this study advances an understanding of oil income as the outcome of deliberate policy choices. In doing so, I bring together many of the policy choices about how to govern natural resource endowments that are typically examined separately. By examining these factors jointly, I show that governments are forced to make trade-offs. For example, governments can increase their participation in oil development (by claiming ownership stakes in projects for NOCs) as a means of increasing their claim to oil rents. In doing so, however, they are also increasing their exposure to the risks of oil exploration and oil price volatility. Or, governments can secure higher investment guarantees from IOCs in order to intensify oil exploration activity, but they must sacrifice their overall claim to oil rents to do so. A key contribution of the paper

is to identify how trade-offs between rent collection, risk exposure, and investment levels work in theory and in practice (section 3). Using original data on 825 historically confidential oil contracts and pieces of legislation governing the hydrocarbons sector from 55 countries (1974-2004), I show empirically that countries that maximize their claims to oil rents do so by increasing their risk exposure and reducing investment levels. This first finding motivates a second question: why do politicians make different trade-offs in designing a regime to govern the oil sector?

I argue that differences in resource governance can be understood more broadly as different balances of risks and rewards over time (section 4). A taxation regime relying more heavily on forms of taxation that are not linked to the profitability of oil discoveries enables governments to collect revenues more quickly and with less risk. However, heavily weighting taxes towards the present constrains the overall amount of taxation that a government can impose while still securing investment in the oil sector, reducing future oil income. Governments able to assume some risk over the level of oil income may have to wait to collect revenues but can end up with higher shares of oil rents and higher shares of windfall profits.

Beyond the importance of oil income itself due to its centrality in comparative politics and its significant share of the global economy, examining how governments make trade-offs over time in oil governance offers an opportunity to test classic theories about what types of leaders are more likely to make policy choices that maximize long-run welfare. In particular, I focus on how political regimes and politicians' security in office shape trade-offs over time in the design of taxation regimes. There are two views on how political regimes may influence taxation regimes. First, democracies and autocracies differ systematically in terms of their ability to make credible commitments not to expropriate from investors. This could reduce the need for democracies to offer tax incentives to investors, for example, and enable them to secure overall better deals vis-a-vis multinational companies. Alternatively, democracies could have lower optimal tax rates than autocracies because they have a more encompassing interest in long-run economic growth. However, differences within regimes can be as important as differences between them in shaping the government's relationship to investors. New democracies may be especially predatory towards private

property (Przeworski 1991), and stable autocracies can successfully protect private property (Olson 1993). Therefore, I also consider the effect of leaders' time horizons on how they balance the risks and rewards of oil development over the long-term.

In evaluating these arguments, I focus exclusively on middle- and low-income (non-OECD), oil-producing countries. Many studies of the effect on oil on political and economic development focus on the difference between Norway and Nigeria in the management of oil endowments, but we know comparatively less about why Colombia manages its resource endowment better than, say, Ecuador. Many new discoveries over the next years will be in new rather than mature producers. These new producers are making policies over resource governance for the first time, and there is significant room for improvement. Paul Collier dramatically claims that "the countries of the bottom billion have one lifeline: nature," or, more precisely, natural resources (2010: p. 3). He argues that optimally managing resource extraction is the biggest development opportunity for the world's poorest countries. Examining differences among developing countries in resource management offers the opportunity to offer more policy insights about how developing countries can improve resource governance with a realistic benchmark.

I find that democracies and autocracies pursue two distinct strategies with regards to developing their oil sector. More democratic countries, that should be better able to commit to long-term contracts and thus secure better up-front deals, in fact set lower tax rates and collect fewer up-front revenues, enabling them to assume lower levels of project risk and to increase the commercial viability of marginal fields. They utilize exogenous increases in their bargaining power to secure higher levels of foreign investment. In other words, they are taking a long view with regards to their oil sector, enacting policies that can increase the overall size of their reserves over time. To do so, they set lower tax rates. More autocratic countries, by contrast, tend to ratchet up tax rates on large oil fields, frequently assuming some of the project risk themselves, often at the expense of investment levels and the commerciality of smaller fields. In other words, they extract more from the existing resource base but they do less to increase the size of the resource base over time.

Further, I find differences within democracies based on the maturity of democratic institutions:

democracies led by more institutionalized political parties (with longer time horizons) are less likely to front-load taxation regimes, enabling them to secure relatively higher overall tax rates compared to democracies led by less mature parties. They are also more likely to design taxation regimes that anticipate potential changes in market conditions and enable governments to reap the benefits of windfall profit scenarios. Instead, autocratic leaders with shorter time horizons use exogenous increases in their bargaining power to front-load taxation regimes rather than to secure more investment. Whereas the benefits of higher tax rates can take years to pay off in the oil sector, acts that front-load tax regimes (e.g. collecting signatory bonuses), by comparison, offer comparatively quick revenue turn-arounds for insecure leaders looking for immediate revenue increases.

Overall, these findings suggest that democracies and autocracies are pursuing fundamentally different strategies in governing natural resources. Democracies tend to manage their resource endowments with an eye towards the long-term, adopting policies that encourage investment in order to increase the size of the resource base over time. More institutionalized democracies pursue this strategy to an even greater extent. By contrast, autocracies tend to focus on extracting higher shares of rents from the existing reserve base, and leaders with shorter time horizons focus even more narrowly on front-loading their claims to rents.

In summary, I address how political regimes and leader time horizons shape natural resource governance. In doing so, I engage several academic and policy debates. First, I establish that governments can and do make policy choices that influence the level of oil income in government coffers both today and in the future, implying that oil income is endogenous to many of the political outcomes that oil is thought to cause. I show that many of the characteristics of oil rents that are theorized to affect political outcomes–including their size, timing, and volatility–are shaped by policy choices. Indeed, many studies of the political economy of oil argue that oil affects political development through its influence on policies without actually examining the policies themselves or acknowledging that political development also influence the policies. Even among studies that explicitly theorize the relationship between oil, political development, and policy choices, the con-

tributions tend to be theoretical rather than empirical (e.g. Robinson et al. 2006). This study both theorizes the relationships and offers new data to illustrate how they work in practice.

Second, I enter into the debates on the role of regime type and time horizons in host country relationships with multinational corporations. Often, these relationships are studied by looking at foreign direct investment (FDI) flows generally or FDI flows within sectors, but this is muddled by differences in quality and price of products produced in different countries. It can be difficult to compare tax regimes across countries that govern essentially different products traded in different markets. By focusing on these relationships in the context of a single commodity traded in a single international market, we can more clearly identify the relationship between political regimes and time horizons and differences in taxation regimes. Further, by examining the full menu of policies that governments use in designing ownership regimes–including government participation, tax levels, and flexibility of tax regimes–rather than focusing on a single dimension, I can elucidate the overall strategies that different governments are using to govern resource endowments. These strategies can be obscured by focusing on a single dimension of revenue collection, such as tax levels. In broadening the scope of FDI policies that I examine, I also offer a different interpretation of government participation than is typically seen in the literature. Rather than viewing government ownership as a zero-sum shift of benefits from multinational companies to host country governments, I argue that government participation also reallocates risk between parties, implying that it is a strategy with both costs and benefits for both parties.

Third, decisions about natural resource governance parallel a broader class of political phenomena. Essentially, deciding how to tax oil production requires balancing short-term against long-term revenues. Governments face many decisions which require balancing the short-term against the long-term—such as, whether to forego current consumption to invest in education, whether to enact costly environmental regulations now in the hopes of reducing the effects of climate change, or whether to undertake costly pension reform (Jacobs 2011). A theory of intertemporal allocation of oil rents should shed light on other policies that require similar trade-offs over time. These questions of time are relatively understudied—but essential—to governance.

The paper proceeds as follows. In section 2, I describe the bargaining environment between host country governments and multinational oil companies, focusing on time inconsistency dilemmas inherent to FDI, particularly in the extractive industries. Given the bargaining environment, section 3 outlines the trade-off between maximizing oil revenues in the short-term and securing a greater share of oil income over the long-term. In doing so, it offers an overview of different tax regime designs and the nuts and bolts of how governments tax the oil sector. I then empirically establish that trade-offs exist in designing tax regimes by analyzing the design of real oil contracts. In the fourth section, I offer a theory for how political regimes and time horizons shape tax regime design based on the time horizons over which the costs and benefits of various tax regime designs will accrue. The fifth section outlines the data collection process, defends the strategy for using the information in oil contracts and legislation to assess tax regimes, and addresses concerns about missing data on oil contracts. Section 6 evaluates the argument. The final section concludes and offers ideas on how the paper advances theoretical and policy debates.

2.2 Oil investments and credibility of commitments

Oil contracts are long-term arrangements between governments and companies negotiated to govern projects with a specific economic structure. In particular, extraction and development of oil requires large outlays of capital during early years. These investments in oil exploration are large and highly specific. It is difficult to use an oil well to do something other than drill for oil, and, once sunk, oil wells are difficult to move. There is then a lag between investments and returns; investors must wait several years before oil discoveries come online and even longer before enough oil has been produced to fully recover sunk costs. And, there is no guarantee that investments in oil exploration will yield returns. Ninety percent of oil exploration efforts result in a loss (Radon 2005: p. 62). Governments typically lack the large sums of capital, the ability to withstand significant risk to this capital, and the technical capacity to undertake these investments on their own. As a result,

governments often rely on international oil corporations (IOCs), which could be privately-owned or publicly-owned entities investing abroad[1], to develop oil endowments.

Having a second party develop oil endowments introduces a conflict of interest. IOCs need to recover the costs of oil exploration and development and wish to retain as many of the rents as possible. Host countries (HCs), on the other hand, want to collect as many of the rents as possible while still attracting and maintaining foreign investment. Additionally, each party would like to secure its goals while absorbing as little risk as possible. When parties are responsible for paying the costs for sinking exploration wells, they assume the risk that the amount of oil discovered will be below what would be necessary for a commercial discovery.

Parties also have to manage oil price volatility. In the past 10 years alone, oil prices have swung from $54 a barrel in 2005 to an all-time high of $147 a barrel in mid-2008 to below $50 barrel today. Take Cote d'Ivoire as an example, a minor oil producer at 37,000 barrels of oil per day. In 2013, the difference between $147 a barrel and $47 a barrel amounts to a difference in gross revenues from oil production of $3.7 million *per day*, or $1.3 trillion per year and 6% of GDP.[2] Governments could collect a fixed amount of revenue each year, forcing IOCs to bear all the risks of price volatility. However, when IOCs bear all the risks from low oil prices, they also capture all the upside when prices rise. Conversely, governments can pay IOCs a fixed amount to extract oil each year, assuming all of the risk of price volatility themselves as well as the opportunity to capture all of the upside from price booms. More commonly, governments and IOCs share the risks of price volatility but to varying extents.

HCs and IOCs negotiate oil contracts that allocate the risks and rewards from oil extraction between parties. A central issue in the negotiation of these contracts is that contracts negotiated

[1] Some nationally-owned oil companies are increasingly behaving like privately-owned oil companies and investing in oil exploration and production outside of their home states. Jones Luong and Sierra argue that whether state-owned oil companies invest abroad depends on whether the company emerged from a consensual or conflictual nationalization process and whether company managers' and governments' interests regarding internationalization converged (forthcoming).

[2] This is an incredible amount of volatility for governments to manage. Indeed, van der Ploeg and Poelhekke (2009) find that natural resources can have a positive effect on growth, but that this effect is overshadowed by a larger, indirect and negative effect of commodity price volatility on growth. This volatility is one of the primary reasons, they argue, that we tend to observe low average growth rates in commodity-exporting developing countries.

with a sovereign government are difficult to enforce. Contracts between private parties can appeal to courts when contractual terms are reneged upon or renegotiated without proper compensation. However, IOCs have limited means to force governments to abide by contractual terms. MNCs can seek international arbitration through the International Chamber of Commerce or the International Centre for the Settlement of Disputes to demand compensation for adverse changes to contractual terms, but state ownership of oil is often enshrined in the constitution, meaning that states can invoke sovereign immunity by arguing that changes to petroleum laws are "an issue of public interest and not subject to foreign arbitration" (Witten 2008-9: p. 72). For example, the Venezuelan Constitution enacted in 1999 directly links hydrocarbons ownership and the public interest:

> [F]ields of minerals and hydrocarbons existing in the national territory, in the territorial sea, in the exclusive economic zone, and on the continental shelf belong to the republic, are under the regime of public ownership, and hence give rise to inalienable and imprescriptible national rights (Constitucion de la Rcpublica Bolivariana de Venezuela art. 12, as cited in Witten 2008-9: p. 72).

Adding to the limited enforceability of contracts, the economic circumstances that shaped the initial contractual agreement shift over the life of an oil field. When initial bargains are struck, investors have the upper hand in bargaining, as host countries can rarely command the capital and technology required for oil development. Because investors are putting their capital at high risk, they require a high rate of return to make this initial outlay worthwhile, forcing host governments to offer attractive terms to attract foreign capital.

Once investments are sunk, however, the initial terms of the bargain to "obsolesce" (Vernon 1971). Since initial capital outlays are sunk costs for companies, companies have incentives to continue to produce oil even if the terms of the initial bargain shift.[3] Further, domestic actors

[3]However, we may overestimate IOCs' willingness to continue to produce when initial contractual terms are adversely changed, and this willingness may vary among IOCs. In September 2006, Venezuelan President Hugo Chavez demanded increased ownership in oil production for Venezuela's state-owned oil company, PDVSA (from an average of 40% to 60-83%). Chevron, Statoil, BP, and Total agreed to these new terms, but ConocoPhillips and ExxonMobil walked away from their investment to seek compensation through international arbitration. This process took 7 years, and ExxonMobil was awarded about 10% of what they claimed the value of the project was ($1.6 versus $16.6 billion) (Vyas and Gilbert 2014).

begin to gain expertise in oil extraction, making threats of full or partial nationalization of the oil industry more credible. Knowing this, host governments can essentially hold investments hostage under the threat of taking full control over the oil industry, and, using this threat, can extract higher shares of profits. Given the shift in bargaining power after the initial bargain is struck combined with lack of contract enforceability, governments have difficulty credibly committing to long-term contractual arrangements.

In theory, the time-inconsistency problems should determine how HCs and IOCs divide profits. If IOCs anticipate expropriation, then they will only offer a contract that compensates them for this risk. However, this only makes expropriation more likely, as such a contract would put governments in the position of receiving especially low gains in a windfall profit environment. Moreover, if IOCs signed the initial terms expecting that the HC would expropriate, then they are essentially receiving a windfall profit if the government fails to expropriate.[4] Thus, it is possible for governments to fall into a cycle. The inability to credibly commit leads to contracts that are more vulnerable to expropriations (because they fail to fairly compensate the government), and a history of expropriations increases government's reputation for expropriations, further reducing the credibility of the governments' commitments (Hogan et al. 2010; Summers 2010). This problem is exacerbated because it is not just that parties must deal with the anticipated shift over time in bargaining power between IOCs and HCs but also with potentially large changes in market conditions (e.g. swings in oil prices, shifts in demand, or supply shocks).

These vicious cycles of granting overly permissive contractual terms, followed by expropriations, followed by even more permissive contractual terms and more expropriations should deter the kind of large investments required to develop oil resources. However, investment in natural resources is even higher in countries with weak administrative capacity and insecure property rights compared to stronger states (Menaldo, forthcoming). One explanation is that an increase in resource reserves creates a more attractive investment climate, allowing governments of resource-rich countries to maintain higher average levels of both expropriation and investment compared

[4]One could argue that if expropriation is anticipated and it is factored into the initial deal, then it is not really expropriation in the sense that an IOCs' assets are seized without fair compensation.

to resource-poor countries (Jensen and Johnston 2011). However, expropriation in a resource-rich country should still reduce investment compared to a counterfactual in the same resource-rich country that never expropriates.[5]

Conflating this discussion is the idea that expropriations contain purely upside for governments and purely downside for IOCs. But, assuming greater ownership stakes in oil development also means assuming some of the risk inherent in oil exploration and development. Even if the exploration costs are already sunk for an expropriated asset, governments often reimburse IOCs for their share of the costs already sunk as well as the book value of the expropriated assets (see Maurer 2013 on compensation in the Mexico nationalizations). Going forward, greater ownership also means less stability of income for governments, more exposure to oil price volatility, and responsibility for remaining capital investments and lifting costs. After partial or full nationalization, to the extent that governments decide to explore and develop any new fields, then they bear exploration risks (which means putting large sums of capital at risk). In other words, expropriations enable government to increase their claim on the profits of oil extraction, but, in doing so, they increase their exposure to risks. So, by expropriating, governments not only potentially reduce future private investments, they also put their own capital at greater risk.

Counterintuitively, expropriations can also provide benefits to IOCs. HCs now bear a portion of the risk and pay some of the costs of oil development. Further, financial arrangements in which the HC takes an ownership stake could actually allow for more credible commitments as compared to an arrangement where the HC has no stake. Government equity stakes function as a progressive form of revenue collection: governments collect higher shares of profits as profits grow.[6] This has two effects, both of which reduce the likelihood that the bargain will be renegotiated. First,

[5]In theory, acts of expropriation in the oil industry could have spillover reputation effects on other industries. Indeed, Jensen and Johnston (2011) argue that natural resource wealth decreases a country's incentives to uphold any contract with private investors (not just in the resource sector) and find that natural resource wealth increases political risks for firms generally. Tomz and Wright (2010), on the other hand, argue that the costs of expropriation are mostly confined within sectors. Expropriations in the oil industry are rarely punished in international debt markets, and vice versa.

[6]This isn't always the case. In some arrangements, state ownership functions financially more similar to a royalty (if the state pays its share of costs out of its share of production after the costs are incurred). I show how different forms of equity affect the progressiveness / regressiveness of the fiscal regime in the Appendix.

governments with higher claims to profits have lower incentive to expropriate (Jensen and Johnston 2011). Second, progressive taxation regimes, in which the governments' claim to profits rises with profits, are more stable because they are less likely to result in a windfall profit situation in which the government is receiving an unfairly small share (Stroebel and van Benthem 2013). Under this logic, expropriations could result in regimes that offer lower but more stable returns for IOCs.

IOCs recognize these benefits. As an example within the extractives sector, Kennecott mining in Chile offered to sell the Chilean Copper Corporation, the state-owned copper company, a 51 percent interest in the El Teniente mine as a "strategy of protection" (Moran 1973: p. 277). Future investments in the mine would be financed from the proceeds of the sale. Thus, Kennecott was able to expand the mine at no additional financial risk to themselves. Further, by offering the government a partial ownership stake in the mine, Kennecott hoped to satisfy the growing demands for national sovereignty in the mining sector while maintaining a return on investment. In fact, Moran argues that increasing government ownership in a joint venture is a good strategy to *protect* corporate assets abroad in the absence of the gunboat diplomacy that once provided private property protections for oil companies operating abroad (ibid.).

Expropriation events offer IOCs opportunities for low risk investments. At the extreme, countries that have fully nationalized oil industries but need private capital to finance exploration can offer risk service contracts. Under these arrangements, HCs pay IOCs a flat fee to explore and develop oil fields, leaving HCs to bear all of the investment risk. Iran's well-known "buyback" contracts functioned like this. Under the buyback contracts, the IOC provides the capital and develops an oil or gas field; they are subsequently repaid from sales revenues based upon an agreed-upon rate of return that is specified within the contract. Once production begins, control of the field reverts to the National Iranian Oil Company. Thus, Iran captures any profits that accrue in excess of the remuneration to the IOC. However, Iran also bears the risk that oil or gas discoveries will be below expectations (they still have to reimburse contractors).[7]

[7]IOCs have argued that they bear risk under the buyback agreements as well. They are responsible for cost overruns beyond the agreed-upon expenditure levels, and they have uncertain payment timelines given that payments are made out of a percentage of production. If production is below expected levels or if oil prices sink too low, this may significantly delay cost reimbursement (van Groenendaal and Mazraati 2006).

The literature on expropriation views it as an opportunistic grab by HCs that financially harms IOCs. As such, it considers only one dimension of the fiscal regimes that govern oil extraction, the division of profits. However, expropriations are also reallocations of risk between parties. Considering risks and rewards together–and the trade-offs that can be made along these dimensions–yields a fuller picture of how HCs vs. IOCs benefit from the development of oil endowments. Looking at both who bears the risk and who captures the rewards also helps explain why IOCs continue to invest in oil development in countries with insecure property rights.

The main focus of this paper is how the fiscal regimes that govern oil extraction–defined within oil contracts between IOCs and HCs as well as by HC laws and regulations–divide risks and rewards between parties. In the following section, I discuss the dimensions of a fiscal regime that confer value to HCs and MNCs, the most common types of contracts, the terms within contracts that determine how the risks and rewards of oil investment are allocated between parties, and the trade-offs between different types of risks and rewards in both theory and practice. In Section 3, I offer an explanation for why governments pursue contractual arrangements with different weightings of risks and rewards.

2.3 Oil income: Risks vs. rewards

2.3.1 *Are governments getting a good deal?*

Oil contracts are notoriously complex. Given the complexity of oil contracts, there is no simple way to tell whether governments received a good value for their resources. There are at least five key dimensions of contracts that reflect the extent to which the HC's interests are secured.[8] However, there are trade-offs among these dimensions, and governments cannot pursue all interests

[8]Some of the value of an oil contract could also be contained within non-fiscal clauses, such as how liabilities are determined for environmental damages, companies' obligations with regards to local employment and training, and utilization of local materials and services. However, these social and environmental obligations are often omitted from contracts or are so vague that it is difficult to assess their value until they are actually litigated and tested through court systems (Kyle 2014).

at once.

2.3.1.1 Government Take

First, the most common statistic used to evaluate contracts is *government take*, the government's share of the economic profits from resource extraction over the life of the contract. In other words, government take is the effective tax rate on (undiscounted) oil rents, reflecting the effects of the many means that governments use to collect revenues, including corporate income tax, royalties, production-sharing, and government participation, among other instruments. Calculating government take is a complicated exercise; it requires making assumptions about production levels, costs of extraction, and oil prices, among other things. Government take can be sensitive to these assumptions, as will be discussed more in depth in Section 4.

Overly high or low government take statistics can be deceptive. The governments' claim to profits influences whether or not an oil discovery is commercial: with very high levels of government take, oil fields will not be developed that would be under different fiscal arrangements. Even if a contract allowed governments to collect 99% of profits, they can only collect these profits if oil is actually developed. Oil would only be developed in that scenario if the field size was very large and the geological and market conditions made profit levels extraordinarily high (either because costs of extraction are very low or prices are very high).

A low government take statistic, on the other hand, can look like a bad deal for governments. However, low tax rates can make marginal discoveries commercial and encourage higher levels of investment, potentially increasing the overall size of discovered reserves down the line. Low government take could be necessary to attract investment when costs are very high or prices very low.[9] Further, even if a particular level of government take could secure a positive net present value for IOCs, they are less induced to invest if the government take means that they will yield a lower return than if they had invested the same capital in another country.

The trade-off between government take and investment can be seen in practice in Ecuador

[9]Chad is often cited as an example of a country that got a bad deal during initial contract negotiations because the government take statistic was lower (around 40%) than other countries. However, this does not take into consideration the fact that the transportation costs in Chad were very high and the quality of oil relatively low (Johnston 2007).

during the recent oil price boom. Between 2006 and 2010, Ecuadorian President Rafael Correa increased the windfall profits tax to 99%, nationalized the assets of a French energy company (Perenco), and undertook a wave of contract renegotiations. These actions increased the governments' claims to oil rents from 70% to 80%. However, the contract renegotiations secured investment obligations of only $1.2 billion in Ecuador's oil industry over the next four years, the same amount that had been invested in 2006 alone. Reduced investment was rapidly reflected in declining oil production: between 2006 and 2010, oil production fell from 255,700 barrels per year to 162,000, a gap worth 4% of GDP (at 2010 oil prices) ("If It Ain't Broke..." 2010).

2.3.1.2 Front-Loaded Payments

Second, governments want to secure their share of the economic profits from oil extraction sooner rather than later. In other words, they care about the extent to which contracts are *front-loaded*. Governments can collect revenues sooner rather than later by demanding signing bonuses and by setting limits on IOCs' ability to recover costs. The timing of revenues can be crucial both economically and politically.

From an economic perspective, governments of developing countries may reasonably have higher discount rates than the IOCs they bargain with. Returns on investment should be higher in a capital poor environment if resource rents are invested in productive areas.[10] Further, consumption today is worth more in countries that are poorer (Collier et al. 2010).

From a political perspective, failing to front-weight revenues can create thorny political dilemmas. It can take years after production begins for IOCs to fully recover exploration costs and move into a tax-paying position. Once oil starts flowing, however, citizens may expect to start benefiting from oil revenues that are not yet materializing in government coffers. Ross (2012) argues that if citizens believe their government is failing to deliver sufficient services relative to the revenues they are collecting, they will seek to replace the ruler, either by rebelling (in autocracies) or voting

[10] If resource-producing countries deplete natural capital more quickly than they build up other forms of capital (e.g. financial, human), then resource extraction makes countries *poorer* over time (Heal 2007). Said another way, resource-rich countries can only maintain current consumption levels if rents from nonrenewable natural resources are continuously invested. This is known as the Hartwick Rule (Hartwick 1977; Solow 1986).

the incumbent out of office (in democracies). When citizens pay taxes, he argues, they can make reasonable assessments of how much the government is collecting in tax revenue relative to how much it is spending. The secrecy of oil revenues, however, enables governments of oil-producing countries, particularly autocratic ones, to collect revenues that citizens cannot directly observe. Thus, citizens could falsely conclude that governments of oil-producing countries are performing well relative to the revenues they are collecting because they are underestimating these revenues (Ross 2012: ch. 3).

However, citizens could just as easily *overestimate* the size of government revenues from oil, causing their demand for government spending to exceed revenues. Jones Luong and Weinthal (2010) cite overly high citizen expectations about their right to benefit from oil revenues as one of the key causes of the resource curse: when resources are state-owned, citizen expectations about benefiting from resource rents escalate, pushing the state to overspend on patronage and resulting in the deterioration of institutions and the economy. Mismatches between citizen expectations and the size of government revenues should be particularly high during early production years under a fiscal regime that fails to front-load taxation obligations. Citizens can observe oil flowing, but do not observe that oil projects are not yet profitable from an accounting perspective.[11]

Collecting revenues up-front is not costless. In theory, taxes that front-weight revenue collection for the government tend to delay capital investment and reduce total investments compared to neutral taxes (Smith 2012). Front-weighting revenue collection can also be problematic because it requires a more difficult intertemporal commitment from the government to the IOC. Resource contracts are negotiated under conditions of imperfect information: neither governments nor companies know for sure whether commercial reserves will be discovered. If no commercial reserves are discovered, then up-front payments to the government can look like a smart strategy because they secured payments to the government even in the absence of a valuable discovery. However, if commercial reserves are discovered, then governments will be incentivized to renegotiate contracts. Companies anticipate the risk of rising tax rates *ex ante*. So, oil contracts are typically

[11]Christensen (2015) applies a similar logic in the mining sector, arguing that overly high citizens expectations about the profits from mining activities sow the seeds for local protests around mining sites.

designed to address this commitment problem by compensating investors *ex ante* for the risks of renegotiation of initial contractual terms, usually by offering generous depletion and cost recovery allowances. To the extent that contracts instead front-load tax obligations, this requires companies to put more initial capital at risk. They could compensate for this either by reducing early capital investments or by demanding lower overall tax obligations.

2.3.1.3 Exploration Risk

Third, contracts allocate *risk* between parties. Significant risk is involved in the hydrocarbons sector, and who bears the risk plays a major role in determining the value of the contract. In general, HCs would like to secure greater shares of profits and secure those shares sooner and at lower levels of risk. IOCs would like to do the same.

Much of the comparative politics literature takes government ownership of natural resources as a foregone conclusion. Governments have long claimed ownership of subsoil mineral deposits; Ross (2012) traces governments' claim to mineral rights back to the Roman Empire (ch. 2). Indeed, one of the defining features of the *rentier* state is that predatory leaders easily capture and control resource rents (e.g. Karl 1997; Mahdavy 1970; Vandewalle 1998). However, it is important not to conflate state ownership with state control (Jones Luong and Weinthal 2010). Even if states own oil endowments in the sense that reserves in the ground are considered government assets and only the government has the right to decide how and when oil is extracted, they may transfer ownership to private companies at the wellhead (as soon as oil leaves the ground), transfer only partial ownership to private companies (by taking an equity stake in a joint venture), or maintain full ownership and control over extraction (by fully nationalizing the oil industry). In fact, very few countries maintain full ownership and control over the entire life cycle of oil from exploration to marketing. Mexico and Saudi Arabia are prominent examples, yet even these countries have utilized private companies as subcontractors for varies parts of the value chain.[12]

Increasing government ownership provides benefits for HCs. They can use greater ownership to

[12]The tendency to over-estimate state ownership can be seen in the fact that empirical studies use the mere *formation* of a state-owned oil company as evidence of expropriation within the oil industry (e.g. Mahdavi 2014).

increase government take in a project. Further, government equity stakes function as a progressive form of revenue collection: governments collect higher shares of profits as profits grow. Government participation in joint ventures with more experienced IOCs could also enhance domestic technical and human capital capacity to explore and develop for oil over the long run.

However, government participation, in addition to increasing the government's claim to profits, also puts government capital at risk. Compared to IOCs, governments of developing countries may have to pay more to borrow this capital and be less able to bear the risk. Different arrangements, even at similar levels of government participation, entail different levels of risk for the government. Under some arrangements, state-owned oil companies participate as an equal partner in oil exploration from day one, covering their portion of exploration and production costs and bearing their share of exploration risk. In this case, governments may face negative income streams during early years, as the majority of capital costs are incurred in the first several years. Alternatively, governments can "carry" a working interest during the exploration phase. In this case, companies are usually responsible for covering exploration costs. Government participation kicks in in the case of a commercial discovery, and state-owned oil companies may then reimburse companies for their share of costs, depending on contractual provisions, either with or without interest.

The extent to which government participation entails risk for the government depends on how participation is arranged within the contract. Contracts requiring state-owned oil companies to cover their share of costs from day one entail more risk than contracts which make payment of costs conditional on a commercial discovery. This has a large impact on the overall front-loadedness of a contract: contracts that delay cost reimbursement are more front-loaded for governments. Over the long run, government participation in oil development can also affect the size of the resource base. If NOCs are less efficient at exploring for and developing oil than IOCs, then government participation in the oil industry could lead to smaller discoveries, higher costs, and lower production levels. Too much NOC participation could also reduce investment levels in oil development if the state lacks the capital to invest at optimal levels[13] or if IOCs invest below

[13]For example, Venezuela reverted all concessions to the state in 1971, dramatically increasing the government's take from oil production. In the near term, oil revenues to the state quadrupled, and companies continued to produce

the levels they would otherwise invest under a similar taxation regime that did not include NOC participation because they simply prefer not to partner with NOCs.

2.3.1.4 Investment Guarantees

Fourth, oil contracts can set *investment requirements*, as well as the time frame under which these investments must be made. At a minimum, these requirements help governments ensure that IOCs do not sign contracts and leave oil fields undeveloped for years. This is important for any government managing an oil endowment on its citizens' behalf. Before the 1970s, it was quite common for governments to sign away the rights to large tracts of land to foreign oil companies, giving them the right, but not the obligation, to explore for oil. Companies could then hold the land for years, at high opportunity cost for the host country.

Investment requirements make this practice costlier: if IOCs fail to invest at the levels specified within the contract, they owe the government the difference between the amount that they have invested and the investment guarantee. Larger investments guarantees can benefit countries because higher investments in oil exploration can yield higher probabilities of oil discovery, potentially increasing the overall size of the oil endowment over the long run.[14] For this reason, minimum investment requirements are often a key bidding item in auctions, with governments judging potential deals based on IOCs' willingness to guarantee higher levels of investment in oil exploration.

Like other contractual terms, investment guarantees are not costless to secure. Lower taxation levels and less front-loading should increase IOCs' willingness to invest. Contracts usually require IOCs to guarantee the investment requirement by depositing the amount in a bank account, which will revert to the government should companies fail to invest at the required levels. Setting overly high investment guarantees reduces the pool of companies that can bid on a project because only a small number of companies can obtain such large sums of capital all at once.[15] Reducing the pool

existing wells. However, investment in new wells declined; over the long term, production levels declined as well, until per capital oil exports today are no more than 15% of what they were in 1970 (Philip 2010).

[14] These investment guarantees serve as a floor for investment; IOCs can always invest more in an oil project.

[15] Setting overly high signatory bonuses has the same effect.

of potential partners reduces the governments' bargaining power on other contractual dimensions.

2.3.1.5 Flexibility

Finally, oil contracts can be assessed based upon their ability to secure governments revenue under a wide variety of market conditions. Oil price volatility confounds the design of fiscal regimes. If fiscal systems are *regressive*, then governments are more protected against the possibility of low oil prices. Claiming higher shares of revenue in low price environments and allowing IOCs to claim higher shares in high price environments reduces governments' exposure to price volatility. This structure would theoretically be valuable to countries with low administrative capacity and low ability to credibly commit to revenue-smoothing through other mechanisms (e.g. by making contributions to natural resource funds).

However, these regressive regimes can result in unfair divisions of resources when oil prices exceed expectations. Consider an example from Humphreys et al. (2007: p. 323): If oil companies were willing to make investments and produce oil when they expect prices to be $30 a barrel and prices unexpectedly escalate to $90 a barrel, then companies are receiving three times the return that would have been required for them to invest. When fiscal regimes are regressive, companies capture these windfall returns. *Progressive* regimes–ones that increase the governments' claim to profits as profits increase–ensure that governments capture some of the upside of these windfalls as well. This can reduce the frequency of bargains that are *ex post* unfair for countries. The fact that governments capture lower shares of profits in high price environments may explain the finding that contract renegotiations and expropriations are more likely during oil price booms (see, e.g., Guriev et al. 2011; Manzano and Monaldi 2008, 2010). Indeed, the perception that the division of profits between parties is unfair may be a key motivator for nationalizations (Mahdavi 2014).

Thus, it is not surprising that contracts with progressive fiscal elements are less likely to be renegotiated than contracts with regressive elements (Stroebel and van Benthem 2013). This goes along with the more general argument that when there is uncertainty, flexible contracts are more optimal than rigid ones (Hart and Moore 2008). Humphreys et al. (2007: ch. 12) identify designing progressive taxation regimes as a priority for "escaping the resource curse."

If parties do not expect prices to rise, then governments should, in theory, be able to write progressive elements into a contract without affecting the value of the deal for IOCs. Thus, in theory, they should not have to sacrifice overall level of government take in order to secure this flexibility. Protecting government revenues when profits are low more obviously affects governments' ability to secure good terms along other dimensions of the contract. Regressive contractual elements tend to be front-loaded, causing investment distortions.

2.3.2 *Fiscal terms that define the allocation of risks and rewards between parties*

Below, I define the primary instruments that governments employ to collect oil revenues and how these instruments allocate risk between parties:

- Signatory bonus: A signatory bonus is a fixed payment made by companies upon the signing of a contract. These payments act as one-time windfalls for government. They receive them irrespective of oil discoveries or field profitability. As such, these payments are highly front-loaded (governments receive them immediately upon signing contracts), and they entail no risk for governments. From the IOC's perspective, signatory bonuses increase the amount of capital they must have on hand (in addition to investment requirements).

- Royalty: A royalty is a tax based on a percentage of production.[16] Because it is based on production rather than profits, royalties are collected irrespective of when (or if) fields become profitable. Thus, they are relatively front-loaded taxes, providing early revenue for governments. Under a royalty system, governments received smaller and more fixed shares of profits, while companies retain larger but more variable shares of profits. Thus, companies retain the majority of the risk from oil price fluctuations.

- Cost recovery limits: After royalties are levied, governments may additionally set limits on

[16] I do not consider surface royalties–royalties based on the amount of land under development rather than on the amount of production–here (also known as "land rentals"). They are relatively uncommon after the 1960s; to the extent that they are still used, they represent a marginal share of government revenues.

the percentage of gross revenues that can be allocated towards recovering the costs from oil production. Recoverable costs consist of depreciation of capital expenses, the largest portion of which are undertaken during the exploration phase, as well as operating costs. A country's accounting regulations set depreciation schedules, which can vary from a relatively accelerated depreciation schedule of 3 years to an extended depreciation schedule of 10 years. Accelerated depreciation schedules enable companies to recover costs more quickly, making projects profitable more quickly. However, they limit revenues to governments in early years since revenues are used for recovering costs (meaning that neither income taxes nor profit oil splits can kick in). The combination of depreciation timelines and cost recovery limits have no impact on government take but play a major role in determining how quickly governments can collect oil income. Low cost recovery limits and long depreciation timelines ensure that governments can collect revenues before fields become profitable.

- Profit oil split: Any oil remaining after deducting royalties and costs can be subject to profit oil splits, or production-sharing. Profit oil splits can be fixed or variable. Fixed profit oil splits are financially equivalent to an income tax (though production-sharing contracts often contain both profit oil splits and income taxes). Variable profit oil splits can be linked to field profitability, e.g. by increasing the government's share of profit oil as the field's rate of return rises, making them progressive taxes. Thus, profit oil splits can be a means to secure more income for the government when prices are high, but they are also less front-loaded (they cannot be collected until a field is profitable) and more risky (government income is more variable).

- Domestic market obligation: Companies' share of profit oil can additionally be subject to domestic market obligations. Under these provisions, companies commit a share of their claim to profit oil to be sold in domestic markets within the host country, usually at a reduced price. For example, in Indonesia during the 1990s, companies were required to sell 7-25% of their share of profit oil on the domestic market at 10-25% of the market price for oil.

This obligation functions as the financial equivalent of a royalty[17], so it offers a means of collecting oil income with relatively low variability for governments. They are most useful to governments that subsidize the domestic price of fuel and would be purchasing fuel products at international prices to sell in domestic markets.

- Service payment: These are payments by governments to companies for services rendered during the oil exploration and production process. Means of calculating service payments vary widely, as does the allocation of risk between parties. In some cases, companies are responsible for bearing exploration risk and only receive compensation when and if oil is discovered and produced in commercial quantities. In other cases, governments reimburse companies for exploration and production costs, subjecting government income to both geological risk and income variability from oil price fluctuations. In the latter case, service payments can mean negative income for governments in early years but can secure high overall shares of profits from oil extraction in the case of commercial production. As an extreme example, Iranian "buy-back" contracts of the mid-1990s provided fixed payments to companies for oil exploration. Under this arrangement, companies receive fixed income (and a fixed return on investment), while the government receives the variable portion. The extent to which this type of arrangement secures high profit shares for the government depends on oil prices and geological conditions[18], entailing high risk for the government.

- Income tax: Company's revenues after deducting royalties, costs, and profit oil splits can be subject to corporate income taxes. When service payments are involved, these could be subject to corporate income taxes as well. Like profit oil splits, corporate income taxes can be fixed or variable, are not front-loaded, and entail more variable income for governments. In some cases, governments impose an additional layer of income tax often called a "windfall profit tax" that raises the rate of taxation when profits from oil production rise above

[17]E.g. A domestic market obligation to sell 20% of production at 80% of market price is the financial equivalent of a 4% royalty.

[18]Specifically, governments secure higher take when profitability rises, whereas companies secure high take under low-profit conditions. In other words, it is the reverse of a royalty system.

particular thresholds.

- Government participation: Many contracts provide the option for NOCs to participate to varying extents in oil exploration and production. Under some arrangements, state-owned oil companies participate as an equal partner in oil exploration from day one, covering their portion of exploration and production costs and bearing their share of geological risk. In this case, governments may face negative income streams during early years, as the majority of capital costs are incurred in the first several years. Alternatively, government participation can function as a working interest. In this case, IOCs are usually responsible for covering exploration costs. Government participation kicks in in the case of a commercial discovery, and NOCs may then reimburse companies for their share of costs, depending on contractual provisions, either with or without interest.

- Minimum work obligations: Contracts typically define minimum work programs (capital investments) that IOCs are obligated to fulfill by a timeline designated within a contract. They could be defined based on a number of wells to be drilled or based on a minimum amount to be invested in a field. Failure to meet minimum work obligations constitutes grounds for HCs to end a contract with an IOC. The IOC would then owe the difference between what they have invested and the investment obligation to the government.

This cursory review of the terms that can be used to collect oil income quickly reveals the complexity of evaluating and comparing contracts. No single term within the contract reveals the share of profits that governments will collect, how quickly they will collect them, nor the amount of risk that they will bear over that income. Rather, one must look at the combination of terms as well as the specific design of each individual term to understand the distribution of risks and rewards set through the contract. Moreover, the overall effect of the terms varies with field conditions. The same terms that secure high overall shares of profits for governments when oil prices are low may secure low shares of profits when prices are high.

Table 2.1: Summary of Fiscal Terms, 1974-2004

Fiscal Term	# Country-Years Utilizing Term (% of sample)	Mean	SD	Min	Max
Signatory Bonus ($ M)	200 (9.6%)	8.9	28.2	0.004	225.0
Royalty (%)	1,462 (70.5%)	12.5	0.08	1.85	54.0
Cost Recovery Limit (%)	983 (47.4%)	50.2	16.8	12.4	95
Profit Oil Split (%)	1,064 (51.3%)	56.0	21.7	3.6	90.4
Income Tax (%)	1,372 (66.2%)	34.3	13.5	1.67	74.3
Government Participation (%)	1,270 (61.2%)	40.5	31.1	2.5	100
Minimum Work ($ M)	207 (10.0%)	61.0	417.5	1.1	6,000

Table 1 reveals that there is considerable variation in the fiscal terms that governments utilize to collect oil revenues. While over 70% of the country-years in the sample utilize royalties as a means of revenue collection, for example, royalty rates range from less than 2% (Nigeria, early-2000s) to 54% (Trinidad and Tobago, early-2000s). Similarly, around two-thirds of the sample collect revenues through corporate income taxes, but the rates range from 1.67% (Malaysia, mid-1990s) to 74.3% (Saudi Arabia, early 1970s). To assess the extent to which governments are securing their interests in contract negotiations, we need to not only know what fiscal terms governments are utilizing, but also the rate defined within the fiscal term.

Even knowing the rates and looking at these terms in isolation, however, can be misleading. For example, though Malaysia set a low income tax rate in the mid-1990s, it actually commanded a government take of around 78% because of a high profit-oil split and a royalty. Thus, to fully understand how governments are managing oil endowments, it is necessary to examine how the fiscal terms work together to shape the government take, the front-loadedness of the tax regime, the government's risk exposure, and the progressivity of the tax regime and to understand how these factors interact with each other and with investment levels.

2.3.3 *Understanding relationships among fiscal terms*

Table 2 examines the relationship among the five dimensions of oil contracts that confer value for governments–government take, front-loadedness, risk, investment guarantees, and flexibility.

In this analysis, I examine whether or not trade-offs really exist between dimensions of contracts. To do so, I use an original dataset of oil contracts and pieces of national legislation that govern the oil sector within oil-producing, non-OECD countries between 1974 and 2004. I fully describe the dataset and data collection process in Section 4.

The unit of observation for this analysis is a set of commercial fiscal terms, whether defined by a contract or a national law. Sets of fiscal terms are at least initially defined by real oil contracts signed between HCs and MNCs. However, countries can then pass national laws that change the fiscal terms within the country. In that case, a new observation is defined, where the fiscal terms are those within the contract but including the adjustment made by law. This yields 779 distinct sets of fiscal terms.[19] Multiple sets of fiscal terms could be observed within a single country in a single year. In regressions that examine investment guarantees, the unit of observation is a contract, as investment guarantees are only defined within contracts and not within laws. In regressions that examine progressiveness of the fiscal terms, it includes only those contracts that are commercial (i.e. yield a positive net present value for MNCs) for both low profitability (costs are 45% of revenues) and high profitability (costs are ~0% of revenues) scenarios. Thus, the number of observations for these sets of analyses are lower.

[19] Although 825 oil contracts and national laws are used to define the sets of fiscal terms, in some cases, fiscal terms are defined across multiple pieces of legislation. Thus, the set of distinct fiscal terms is lower than the number of contracts and legislation used to code them.

Table 2.2: Trade-offs Among Fiscal Terms

	Gov't Take *(% profits)*	Front-Loading *P(1st) / P(8th)*	Gov't Risk *(% ownership)*	Investment *(min. inv. / oil prod.)*	Flexibility *(take high profit - take low profit)*
Gov't Take		-0.24 (0.06)**	0.22 (0.04)**	-0.23 (0.10)*	0.18 (0.03)**
N		668	668	412	356
Front-Loading	-0.11 (0.02)**		-0.11 (0.023)**	-0.07 (0.06)	-0.09 (0.02)**
N	668		668	412	356
Gov't Risk	0.17 (0.03)**	-0.20 (0.05)**		-0.12 (0.18)	0.03 (0.03)
N	668	668		412	356
Investment	-0.06 (0.02)*	-0.04 (0.04)	-0.01 (0.01)		0.02 (0.03)
N	412	412	412		197
Flexibility	0.45 (0.08)**	-0.52 (0.13)**	0.11 (0.10)	0.18 (0.20)	
N	356	356	356	197	

*Note: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Table 2 shows the results from 20 separate bivariate regressions. For each regression, the dependent variable is reported at the top of the column and the independent variables in the rows. For each contractual term, the term is calculated using the "most-likely" scenario for oil prices and costs of extraction for each contract, as described in Section 4. Observations are sets of fiscal terms, either oil contracts signed between HCs and IOCs or national laws that fully define fiscal terms. Standard errors in parentheses.*

Several things can be noted from Table 2. First, there is a significant trade-off between contract front-loadedness and government take. The earlier in the life of an oil field the government collects its share of revenue, the lower the overall tax rate that they can collect over the life of a field. If governments of poor countries were making this trade-off in order to make high-return investments in human and physical capital, then perhaps it would be worthwhile to make this trade-off. However, if governments are front-loading fiscal regimes and squandering the revenues on patronage or personal gain, then this is especially bad for the populations of resource-producing countries: not only are governments wasting oil revenues, but they are giving up some of the country's share of oil revenues in order to waste them more quickly.

Second, there is a trade-off between the minimum investment guarantees that governments can

secure within contracts and government take. Higher tax rates yield lower investment guarantees. This is not surprising, but it is important for policy. Some countries demand high levels of government take, but risk declining production levels over the long-term. They get a larger share of a shrinking pie. Other countries, it seems, demand lower shares of economic profits but secure higher levels of investment, potentially growing the size of the oil endowment over the long-term. Which is better is a matter of perspective. Oil is a nonrenewable resource: governments only have one chance to capture its value as it is being extracted. It could be a reasonable position to claim the highest share of profits possible for the country's endowment as it is being extracted. But, it is difficult for governments to capture these high shares without sacrificing investments in reserve replacements.

Third, governments gain larger overall shares of profits when they assume some of the exploration risk. Again, this is a trade-off: governments are increasing their take but also increasing their risk exposure.[20]We can see that progressivity and front-loadedness of the regime are also negatively correlated: this is because methods of collecting revenues early are also regressive (i.e. they can be collected when profits are low). This is the cost of setting royalties, for example. Royalties are helpful to secure the government early revenues, but they put the government in a poor position when windfall profit scenarios arise.

Overall, these correlations confirm reasonable intuitions about the relationship between tax rates and investments. However, these relationships have not previously been established empirically as there is so little publicly available information on the fiscal terms for oil extraction. Secrecy has been one of the hallmarks of oil contract negotiations (Rosenblum and Maples 2009). Indeed, contract transparency is now at the center of the global transparency movement in the extractives industry, with the International Monetary Fund (2007), the Extractives Industries Transparency Initiative (2013), Publish What You Pay (2013), and the European Bank for Reconstruction and Development (2013) all identifying contract transparency as essential for good resource gover-

[20]It is surprising that assuming risk is not also ensuring that the overall taxation regime is progressive. This is because governments are using multiple and offsetting taxation methods to collect revenues. By combining equity stakes with royalties, for example, the government can reduce the progressive effect of the equity stake.

nance.

Beyond providing heretofore unknown information about the structure of oil contracts, these relationships also paint a different characterization of oil rents than is typically taken within the literature. Whereas much of the literature on oil rents assumes that rents flow easily into government hands, the fact that there are significant trade-offs in designing a tax system illustrates that collecting rents is in fact a challenging endeavor. Even among studies that recognize that governments can take deliberate efforts to increase oil rents, it is often assumed that it is easy for governments to do so (e.g. Menaldo, forthcoming). In fact, governments can grab early revenues in the form of signing bonuses and royalties, but they will be sacrificing on other dimensions.

2.4 Political Regimes, Time Horizons, and Oil Income

The finding that governments make trade-offs between risks, total rewards, and timing of rewards begs the question of why they make these trade-offs. To explain the design of fiscal terms, I focus, first, on political regimes and, second, on differing preferences among political leaders over the structure of fiscal terms. I argue that political regimes exert an indeterminate effect on tax levels. On the one hand, democracies should be able to make more credible commitments to investors, giving them more bargaining power to secure better overall deals vis-a-vis multinational companies. On the other hand, democracies should have lower optimal tax rates than autocracies because they prevent significant extraction of surplus by their leaders. However, differing preferences among governments can induce them to make trade-offs between different dimensions of contract value. I argue that time horizons shape these preferences.

2.4.1 *Political Regimes: How do democracies and autocracies differ in natural resource governance?*

A significant literature in international political economy considers the effect of regime type on FDI. Time-inconsistency dilemmas, highlighted in section 1, provide insight into why political institutions shape relationships between host country governments and multinational companies.

Once investments are sunk, firms are left vulnerable to policy changes that could adversely affect returns on investment (Vernon 1971). The fact that firms' elasticity to taxation reduces once investment decisions have been made leaves them vulnerable to contract renegotiations and asset expropriations. These concerns are heightened in the extractives industry due to the high levels and specificity of investments required to develop natural resources. Vulnerability to opportunistic behavior is factored into investment decisions (Kobrin 1984). The anticipation of future opportunistic behavior by governments by firms means that governments will have to offer favorable terms to attract investors; otherwise, they would not invest at all. Prominent theories exist linking regime type to political risk for investors, yet they disagree on exactly how HC-IOC relations would vary across regime types.

In the first line of thinking, all else equal, democracies should be better able to make credible commitments to foreign firms. Democratic institutions increase the number of veto points, reducing the power of the executive to unilaterally change policies. Raising the costs of policy reversal makes commitments to rule of law and protection of private property more credible (Henisz 2002; Jensen 2003). This reduces risks for foreign investors (Jensen 2008). It also extends the timeline over which parties can confidently make contracts. To the extent that democracies offer more policy credibility and better protections to investors[21], they should better be able to bargain for higher fiscal terms vis-a-vis foreign investors. Using this logic, Li argues that "lower levels of [tax] incentives in more democratic hosts are as attractive as higher levels of [tax] incentives in less democratic hosts" (2006: p. 64). In essence, democracies offer better institutional environments for investment, and better institutional environments increase host country bargaining power (Kobrin 1987). This should enable democracies to command higher overall tax rates and better deals vis-a-vis IOCs.

[21]The idea that democracies offer greater policy stability is by no means settled. Normal electoral turnover can result in unstable policies. Politicians seeking to tie the hands of future governments may increase government debt (Alesina and Tabellini 1990; Persson and Svensson 1989), increase withdrawals from natural resource savings funds (Humphreys and Sandbu 2007), or even manipulate oil rents (Dunning 2010). Thus, it is not obvious that democratic institutions in fact provide policy stability. However, democracies do offer more transparency over potential policy changes compared to autocracies (Hollyer et al. 2011). So, IOCs attentive to policy changes would likely see these debated within legislatures and subject to public debate within democracies, whereas autocracies could more easily enact "surprise" policy reversals.

However, the same qualities of democracies that should enable them to better bargain with multinational companies also reduce their incentives to over-extract from the resource sector. Whereas autocratic leaders may seek to grab whatever they can from the resource sector without regard to future consequences for government oil income, democratic leaders, with more encompassing interests in the health of the economy, should limit taxation to levels that can sustain investment levels and the resource base over time. The effect of democratic institutions in limiting taxation levels may be heightened in the resource sector compared to other sectors, as resource rents are immobile and more easily taxable. This means that an autocratic leader–even one with long time horizons (Olson's "stationary bandits")–of a resource-rich state can generate significant tax revenues over the short- and medium-term without developing the economy (Acemoglu et al. 2000).[22]

The effect of political regimes could thus pull in two directions. On the one hand, democratic institutions could enhance investment climates, enhancing HC bargaining power and improving the overall bargain that democracies can obtain compared to autocracies. This should enable them to capture higher government take, more front-loaded contracts, and more progressive tax regimes with less risk and at higher levels of investment compared to autocrats. On the other hand, democratic institutions can enhance incentives to provide economic growth over long time horizons. This could mitigate incentives to increase tax rates. Instead, democracies would lower tax rates and reduce forms of taxation that are distortive to investment (like front-loaded taxes) in order to increase investment in the resource sector, potentially increasing the size of the resource base over time. Lowering the tax rate also serves to increase the size of commercial investments that can be made in the oil sector in the present, as the tax rate influences whether or not developing marginal oil fields will yield a positive net present value for investors.

2.4.2 *Time Horizons: Differences within regimes*

To explain governments' preferences over the design of fiscal regimes (and the trade-offs they

[22]Over the long term, reduced investment levels resulting from high tax levels would translate into lower oil production and fewer new oil discoveries, reducing oil income and the size of the resource base.

make between different elements of the regime), I further emphasize differences in the timing of the costs and benefits that accrue to governments for each of the 5 dimensions of contract regimes that I describe above. For example, securing higher levels of investment guarantees provides a benefit to HCs, but these benefits will only accrue over time. And, if HCs had to reduce the front-loadedness of contracts in order to secure the higher investment guarantee, the opportunity cost of foregone revenue would be born in the near term. I argue that when governments place a higher value on the long-run, they will design fiscal regimes that have more benefits over the entire life of the contract, and that the sensitivity to long-run payoffs increases with the length of the governments' time horizon. By contrast, governments with shorter time horizons will design fiscal regimes that ensure greater benefits in the near term, even if the overall deal leaves countries worse off in the long run.

A leader's incentive to design different fiscal regimes depends, first, on the difference in costs and gains between different financial arrangements and, second, on the timing of those costs and gains. If the government finds the short run benefits of a large signing bonus to be more attractive than the long run benefits of higher investment levels, then they have an incentive to design a front-weighted fiscal regime. Hence, the leader's time horizons influences the incentives to design fiscal regimes with benefits that concentrate in the short- vs. long-run.

A long literature identifies time horizons as critical in shaping government preferences and, through shaping preferences, in shaping government policies. A government's time horizon is the length of time over which it considers the costs and benefits of its policy choices. Government time horizons come from their expectations about being in power in the future as well as their expectations about the possibility of return to power in the future, should they lose office. These expectations are shaped by the institutions that govern executive turnover as well as domestic politics. I thus measure time horizons separately in democracies vs. non-democracies.

Within democracies, I focus on the time horizon of the political party rather than of the individual leader.[23] The extended time that it takes between developing a fiscal regime, negotiating

[23]In a study of how time horizons affect the design of bilateral investment treaties, Blake (2013) also argues that within democracies, the time horizons of political parties rather than of individual leaders shape time horizons.

and signing contracts, sinking investments, and reaching production mean that, under democratic regimes, fiscal regimes developed today may only begin to yield revenues across election cycles. No leader can stay in power forever. However, in democracies, the ruling parties that leaders represent can regain office, even if the current incumbent loses. Not all parties face equal opportunities of regaining office. Some parties are long-lived, while others dissipate when the incumbent loses office.

Huntington (1968) famously argued that parties are key in organizing mass involvement in mobilized societies; without them, societies risk turmoil. Huntington laid out three dimensions of party institutionalization: adaptability, complexity, and autonomy. Fundamentally, an institutionalized party is one which has faced (and survived) a range of difference contingencies and scenarios, one which has multiple overlapping generations of cohorts, and are not beholden to a single family or social group (ibid.). These features are most easily indicated by the party's age: "the logic is that... effective institutions grow slowly, and the older an organization is, the more likely it is to endure even longer" (Dix 1992: p. 491). A party formed in the more distant past is more likely to have faced and survived a variety of political and economic conditions. Older parties are more likely to have be organizations of overlapping generations, with some party members nearing the end of their career and others just beginning, extending the time horizons of the organization (Bates and Shepsle 1997).

Within autocracies, by contrast, the chances of successfully returning to office after losing it are substantially diminished. Thus, the time horizons of autocratic governments are more likely to be aligned with its expectations about remaining in office. Most notably, Mancur Olson (1993) argues that the time horizons faced by autocratic leaders determines whether they establish efficient or extractive institutions. If leaders have a long expected tenure in office, they will behave as "stationary bandits," setting up efficient institutions to promote economic growth over the long-run as a means of increasing the tax base. Leaders with a short expected tenure in office, by contrast, behave as "roving bandits." Their short expected hold in power motivates them to set up extractive institutions to facilitate private gain in the short-term. Leaders with longer tenures in office expect

to gain more from future interactions with oil companies, preventing them from seeking short-term gains at the expense of the long-term.

Whereas the age of parties in democracies is linked to the possibility of returning to power, the age of the regime in autocracies does not link straightforwardly with time horizons. Time horizons within autocracies may be independent of the age of the regime. An autocratic leader gaining office on the back of a powerful anti-colonial revolution may be quite secure in office in his first year, whereas a leader with many years in office could face insecurity from challengers. Thus, I follow Wright (2008) and Blake (2013) in additionally measuring the time horizons of an autocratic government based on the perceived security in office. To proxy for security in office, I measure the probability of regime survival using institutional and environmental factors which correlate with autocratic regime survival. The measurement strategy is discussed in Section 4.

2.4.3 *Hypotheses*

In sum, I predict that both political regimes and government time horizons will influence how governments collect oil income. The effect of political regimes could cut both ways, with more democratic countries either setting higher tax rates due to better policy credibility and enhanced bargaining power or setting lower tax rates due to greater sensitivity to the size of future oil income. The effect of government time horizons is more straightforward: Governments with longer time horizons will prioritize elements of the fiscal regime that enhance government revenues over the long-term, while governments with shorter horizons will prioritize elements of the fiscal regime that help to secure early returns. The main hypotheses are thus:

Hypothesis 1: Democratic institutions will help countries to secure more favorable terms with investors, including higher government take, more front-loadedness, more progressive tax regimes, higher investment levels, and less project risk.

Hypothesis 2: Democratic institutions will incentivize countries to reduce claims on oil rents and minimize taxes that distort investment (i.e. front-loaded taxes) to enhance investment in the oil sector.

Hypothesis 3: Governments with shorter time horizons will favor fiscal terms will prioritize fiscal terms that guarantee early revenues, including more front-loadedness and less take, less investment, less risk, and less progressive taxation.

Hypothesis 3a: Democratic governments with a less institutionalized incumbent political party

will design fiscal regimes that prioritize early revenues.

Hypothesis 3b: Autocratic governments with lower probabilities of survival will design fiscal regimes that prioritize early revenues.

2.4.4 *Alternative Explanations*

There are several potential objections to the explanation presented here. Even if we accept that oil rents are the product of policy choices, the incentives to shift policies could be unrelated to regime type or time horizons. I identify other sources of incentives for governments to vary oil rents and discuss how I account for these in the empirical models.

First, the attractiveness of the investment site, beyond the institutions that govern it, may shape the government's ability to claim oil rents. Namely, a higher quality of the oil endowment should attract higher investment levels at a higher tax rate compared to a lower quality oil endowment. To the extent that the quality of the endowment varies with regime type, this would confound my analysis.

The size and quality of a country's oil endowment at a given period depends on its history of production. Investment in exploration can increase the value of an oil endowment over the long term, even if the size of discoveries diminishes over the lifespan of oil production (Pindyck 1978). This is reflected in the fact that the value per square kilometer of subsoil assets in OECD countries—with long histories of oil production—is $114,000 whereas the value per square kilometer of subsoil assets in Africa—with shorter histories of oil production—is a mere $23,000 (Collier 2010: p. 66). This difference is even more stark when we consider that OECD countries have been depleting their assets for much longer and that the difference cannot be accounted for by geological chance (ibid.). Rising prices and technology can also render resource basins commercially exploitable that were not before. "In reality, there is no 'fixed' reserve base (in an economically meaningful sense) for any resource. If the price of oil were to rise to $200 per barrel (and the demand for oil did not drop to zero), oil would probably be found in some rather strange places" (Pindyck 1978: p. 2). In other words, we should not think of the size of a resource endowment as a fixed element but rather as something that may increase or decrease over time depending on

investment in exploration and discovery (Wright and Czeluska 2007).

There is less geological risk in investing in a country with more proven reserves, or even in a region with more proven reserves, than in an unproven region (Smith and Wells 1993). Lower levels of geological risk imply that investors will be willing to accept a greater share of the total project risk. To see this, consider an example. Assume that exploration costs are $10 million and, conditional on discovery, the expected revenues from oil production are $40 million. The company receives the same expected returns from an investment with an 80% likelihood of making a commercial discovery when it assumes 100% of the exploration costs as when it faces a 60% likelihood of making a commercial discovery when it assumes 20% of the exploration costs.[24] All else equal, countries with more proven reserves and longer histories of oil production–which mean less uncertainty about the size and quality of oil endowments–should secure better deals.

While the size of the oil endowment and the history of exploration are related to the government's claim to oil rents, there is still considerable unexplained variation in government take, even accounting for other features that shape the attractiveness of oil endowments, like costs of extraction. Overall, these factors explain less than 10% of the variation in government take statistics across countries. Figure 1 graphs the residuals from a regression of size of oil reserves, years of oil production, costs of oil extraction, and whether oil resources are primarily onshore or offshore on government take. It reveals that looking at the attractiveness of the oil endowment alone leads to both large positive and large negative differences between observed and expected values of government take. I control for these factors in my empirical analysis. I also examine the effect of within-country changes in regimes through fixed effects models that hold constant features of the country's oil endowments that do not change over time, enhancing confidence in results.

[24]Return = probability of discovery * ($40 - $10) + (1-probability of discovery) * (-$10).

Figure 2.1: Variation in Government Take, Controlling for Quality of Oil Endowment

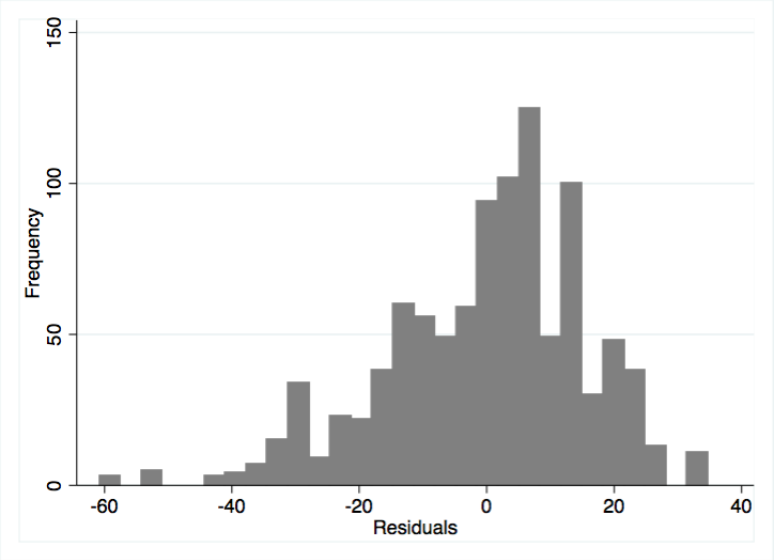


Figure 1 graphs the residuals from a regression of (logged) size of proved oil reserves, total years of oil production, and costs of extraction on government take using the "most-likely" scenario described in Section 4.

Second, many scholars have noted that governments tend to increase their share of rents as oil prices rises and to decrease their share of rents as oil prices decline. If countries nationalize when oil prices are high to increase their claim to rents (e.g. Guriev et al. 2011), they then decrease their share of rents when oil prices are low in order to attract investment (Monaldi 2002). This would be a rational response to the fact that oil prices and tax rates jointly determine whether oil fields are commercially producible. When oil prices are high, tax rates can be high as well, and fields can still yield positive returns for investors. When oil prices are low, by contrast, the same tax rate can render marginal fields uncommercial. Thus, to stabilize production levels, governments would adjust tax rates accordingly. To the extent that oil prices are related to regime type (e.g. Dunning 2008), this would confound the ability to separately examine the effect of regime type on taxation strategies.

There are several reasons to think that governments are not just adjusting tax rates in response to changes in oil prices, however. For one, decreases in the governments' claim to rents are nearly as prevalent as increases between 1974 and 1986, a high oil price environment (Figure 2). Further, when oil prices are low, increases in claims to oil rents are far more prevalent than decreases.

This suggests that, in changing their claim to oil rents, governments are responding to more than changes in prices.

Figure 2.2: Changes in Claims on Oil Rents, High vs. Low Oil Prices

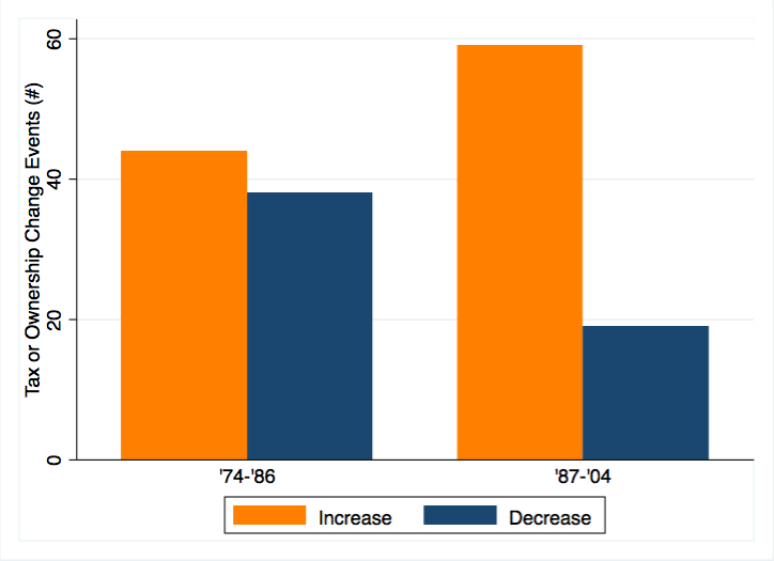


Figure 2 shows the frequency of increases vs. decreases within countries in either level of ownership or tax rates in high vs. low oil price environments. Ownership increases are coded by combining information from coding national laws and oil contracts as described in Section 4 and data on nationalizations from Guriev et al. (2011). Other changes are coded using information from national laws and oil contracts presented in Section 4. A change in tax rate or ownership level could be reflected either by the passage of new hydrocarbons legislation or by the signing of new contracts on different terms. In other words, tax and ownership increases do not necessarily signal expropriations.

Studies of oil expropriations do not typically find this because they focus solely on increases in ownership stakes that adversely affect current investors. However, this is not the only way that governments can increase their claim to oil rents. They can also conduct "creeping expropriation," i.e. increasing tax rates on existing contracts without nationalizing, or offer new contracts under different terms. This is not observed by expropriation studies because they typically only have information on public expropriation events and not on oil contracts. Further, studies looking at the relationship between oil prices and ownership often just look at increases and not decreases (e.g. Guriev et al. 2011). In the Appendix, I show that the type of act (increase vs. decrease) also relates to the way it is pursued (through public laws or through private contracts). Increases in the government's claim to rents are more often pursued through public changes in laws while decreases in the government's claim to rents are more often pursued through more easily concealed

contracts.

A second way of looking at this is by whether countries increase oil production, another means of increasing oil rents, in response to increases in oil prices. However, countries vary substantially in how production levels respond to changes in oil prices, with some countries increasing production levels with prices (e.g. Trinidad and Tobago) and some countries decreasing production levels with prices (e.g. Venezuela).[25] These examples suggest that at least some of the policy choices regarding oil rents must be explained by factors beyond fluctuations in oil prices and the need to attract investment. However, I also control for oil prices in the empirical analysis.

Third, it is possible it is countries' varying reputations for expropriation, which vary with regime type, that shape bargaining outcomes with IOCs rather than regime type itself. IOCs may punish countries with histories of expropriation within the oil sector, irrespective of whether they have democratic institutions or not. Thus, I control separately for histories of expropriation in the analysis.

2.4.5 *Reverse Causation*

Most seriously, relationships between political regime or expected leader tenure and taxation strategies may suffer from reverse causation: the taxation strategies that countries employ vis-a-vis the oil sector could themselves cause political regimes. Whether or not oil rents affect political regimes is hotly contested in the literature (see, for example Haber and Menaldo 2011, Andersen and Ross 2014, and Ross 2012). However, if higher levels of oil rents are indeed associated with reduced likelihood of democratic transitions and greater autocratic longevity, then policy choices promoting higher claims to oil rents could reduce democratic transitions.

I utilize several strategies to mitigate concerns that reverse causation alone is shaping results. First, measures of political regimes and time horizons are lagged in the models.[26] Second, I show the results of fixed effects models, which rely only on the variation in regime type within countries

[25] See Appendix for graphs of production vs. prices.

[26] As an exception, I do not lag the predicted probability of autocratic regime failure, which I use as one of the measures of time horizons within autocracies. This is because I expected that year-to-year changes in expected regime failure incentivize immediate changes in behavior (e.g. opportunistic resource grabs to buy support or to fund repression).

to explain outcomes. This allows us to see whether changes within countries in political institutions affect differences in the various dimensions of tax systems in the following year. Indeed, Haber and Menaldo (2011) argue that including country fixed effects eliminates the correlation between oil rents and regime type. In other words, concerns about reverse causation between oil rents and political development should be less severe when looking just at variation within countries.

Third, I examine the interaction effect of time horizons with oil prices. Whereas a fixed effects estimator can help identify the relationship between political regimes and fiscal regimes by relying on differences in levels of democratic institutions between countries, a fixed effects estimator forces us to bypass the most interesting potential effect of time horizons. Fixed effects would ask, in essence, how does an additional year in party age in the same country shape policy choices, rather than how differences between countries in party institutionalization shape policy choices. Nearly 90% of the overall variation in party institutionalization is between rather than within countries. Instead, I take a different strategy to address the fact that leader tenure may be endogenous to oil rents. Higher oil prices should (exogenously) increase the bargaining power of all HCs relative to IOCs: they can command higher tax rates while holding returns to the investor constant. Essentially, this analysis asks: Does a difference in government time horizons have a different effect depending upon different levels of HC bargaining power? Then, rather than looking at the direct effects of time horizons, I look at whether differences in time horizons differentially shape how countries react to improvements in their bargaining position. Given bargaining power, how do time horizons affect what governments do with it? The potential for reverse causation should be less serious in examining this interaction effect as compared to the direct effects.

2.5 Data and Measurement

In order to evaluate the hypotheses, a database was created of fiscal regimes governing the oil sector within non-OECD oil-producing countries between 1974 and 2004. This time period covers both boom and bust periods in the global oil industry; thus, it covers fiscal regimes designed under a wide range of market conditions. I define oil-producing countries based on whether they have

ever produced more than or equal to $100 per capita (measured in 2013 dollars) per year between 1974 and the present. If they have ever produced this amount, then I include them in the sample for the entire period. Including countries that eventually produce $100 per capita in oil per year[27] but have not yet reached that level enables me to include countries with a wide range of geological uncertainty in the sample with both countries that have well-explored and well-defined oil reserves and countries with relatively unexplored basins. This yields a sample of 56 countries (listed in the Appendix).

The unit of observation for the analysis is the country-year. For each country-year in the sample, I gather information on the fiscal regimes governing oil extraction. Fiscal regimes are defined both within the oil contracts signed between HCs and IOCs and within national laws (often within both). Resource exploration and production is supported by tens (if not hundreds) of contracts between contractors and subcontractors, between contracts and financing institutions, and between contractors and governments. Fiscal terms for oil extraction, however, are defined within the primary contract between the state and the companies involved in extraction. Primary contracts can vary from 10-page agreements that define the obligations of states and companies by referencing existing laws to 150-page agreements in which every term is separately specified within the contract. These contracts can comprehensively cover most of the issues that may arise with oil exploration and production, or can remain silent on key issues such as whether governments are committing to tax stabilization over the life of the contract, how the oil price will be calculated to determine a company's taxable income, and who is liable for environmental cleanups.

Typically, it is necessary to know both the contents of the fiscal terms within primary contracts as well as the fiscal terms within the national laws that govern the private sector generally and the resource sector specifically. For example, the government may determine in a national hydrocarbons law that the NOC is authorized to enter into production-sharing agreements with IOCs to develop oil. In a separate law, the government will define the corporate income tax and the depreciation schedule for capital investments that affect an IOC investing in the country in any sector. A

[27] I adopt this cutoff from Ross (2012).

second law may define extra taxes that apply only to oil companies, such as windfall profit taxes.[28] However, it is typically only within the contract itself that the profit-oil split and cost recovery allowances are defined. Different countries use different combinations of laws and contracts to govern oil extraction. Thus, to get a full picture of the fiscal regime, it is necessary to examine all of the laws that govern foreign direct investment generally as well as foreign investment in the oil sector specifically as well as the primary contract between HCs and IOCs.

These primary contracts have historically been confidential and remain confidential in many oil-producing countries. Thus, defining the full universe of fiscal terms that have ever been signed within every country that has ever explored for oil is a difficult, if not impossible, exercise. No international records exist of all primary contracts setting the fiscal terms for oil extraction ever signed between governments and contractors. Adding to the complexity is that governments may enable multiple fiscal terms during a single time period within the same country. For example, governments often offer different fiscal terms for marginal fields vs. producing fields with existing pipeline infrastructure or for onshore vs. offshore fields.

I gather these contracts from two sources. First, I utilize the fiscal terms from specific contracts and national laws as published in Johnston (2001). In the source, Johnston defines each of the fiscal terms that affect the value of the contract. I rely on his interpretation of the contractual language, and how this language defines the fiscal terms in each contract. The fiscal terms contained in this volume come from the author's consulting business, which consults governments and companies on how to economically model the fiscal regimes governing oil extraction. The source contains a total of 283 sets of fiscal terms covering a total of 143 distinct country-years.[29] These are listed in the Appendix.

Second, I utilize the Basic Oil Laws & Concession Contracts collection at the Barrows Company, an international reference library for oil, gas, and mineral laws and contracts.[30] The collec-

[28]Defining more of the obligations within national laws rather than contracts may be preferable. When every fiscal term and social and enviromental obligation is defined within contracts, this could encourage horse-trading during contractual negotiations (e.g., offsetting higher environmental obligations with lower government take) (Radon 2007).

[29]The source contains more fiscal terms that do not apply to the set of countries and time period considered here.

[30]I thank the Columbia Law Library and the Library of Congress for providing access to this source.

tion contains the complete texts of petroleum laws and contracts. I reviewed 1,369 national laws and 996 oil contracts from the collection by hand to determine whether the document was relevant to understand the country's fiscal regime (e.g. does the contract govern upstream or downstream activities, does the law contain fiscal terms that affect the oil industry, signed during the relevant time period). From this, I culled a total of 345 relevant contracts signed between HCs and IOCs covering a total of 235 distinct country-years. I included all contracts signed between HCs in the sample of non-OECD oil-producing countries between 1974 and 2004, inclusive. In case the HC signed no contracts in 1974, I included the contract signed most recently before 1974 in order to define the status quo fiscal regime in 1974. Additionally, I culled a total of 197 national laws passed by HCs that affected the fiscal terms facing IOCs between 1974 and 2004. These laws include expropriation events, such as increases in government participation and changes in the tax rate, that affect the fiscal regime. I read each of these laws and contracts individually and coded the fiscal terms by hand. I reviewed the codebook with industry experts and legal scholars to ensure that it accurately reflects how both companies and countries understand fiscal terms.[31]

In all, this yields a total of 779 separate sets of fiscal terms. To get a sense for how the dataset is created, it helps to use an example. Imagine a country signs 3 contracts in a given year. For this year, I take the average fiscal terms to define the country's "fiscal regime." By this, I mean I take the average of the government take statistic and not the average profit oil split, for example. Then, this government take statistic is carried forward until the government changes the fiscal regime, either through enacting a new law or by signing new contracts. Now lets say the government enacts a higher income tax but does not change any other fiscal terms. I then reevaluate the 3 contracts with the same fiscal terms, changing only the income tax. I then take the average of those calculations to define the fiscal regime in the year the government passes the new law.

2.5.1 *Missing Data?*

Oil contracts and the fiscal terms within them are notoriously secret. One of the reasons that

[31] I am particularly grateful to the Vale Center for Sustainable Investment at Columbia Law School and to David Johnston for valuable comments. Codebook available upon request.

oil revenues are so valuable to governments is that their size is easily concealed through keeping contracts and fiscal terms confidential (Ross 2012). Thus, a concern with the data is that only contracts from more transparent states are publicly available, or, that only contracts that reflect better (or worse) deals for governments are available, depending on whether governments are more concerned about getting punished for bad deals or about concealing revenues. How can we be confident that the contracts collected here really represent the fiscal terms for the countries in the sample?

First, I do not rely on government disclosure of fiscal terms. Both sources utilized were collected by consulting companies through MNCs for the purpose of advising other MNCs are investment climates and fiscal terms in HCs. Thus, the sources had a financial incentive to get fiscal terms right. Second, by using multiple sources, I can estimate the rate of missing data and look for bias in the sample.

To estimate the rate of missing data, I use a method advocated by Hendrix and Salehyan (2015) to assess underreporting in event data. Essentially, one of the sources can observe a contract and include it in their database. I look at whether the sources capture the contractual terms from a given bidding round, and not whether each source captures the same contracts within each bidding round.[32] These sources can be thought of as independent sources about the underlying population of interest, the world of fiscal terms that have been signed over the time period. We can observe how many sets of fiscal terms are included by each source but also whether some are included by both sources. This information can be used to calculated the rate of missingness in both databases as well as the overall rate of missingness. I examine the missingness for the period between 1980 and 2000, as this is the period covered by both databases.

Between 1980 and 2000, the Johnston database covered fiscal terms in 143 distinct sets of fiscal

[32]There is relatively little variation in fiscal terms within single bidding rounds. In other words, there is little variation between contracts when multiple contracts are signed in the same year. For example, across the entire bidding round in Venezuela's famous 1996 *apertura,* estimated government take between the lowest and highest contract was less than 3 percentage points. Similarly, within the complete 1999 deepwater bidding round in Brazil, the minimum and maximum government take statistics varied by less than one percentage point (authors' calculations). Further, the sources are going to necessarily capture different contracts within the same bidding rounds because they are working with different clients.

terms (41 detected by Johnston + 102 detected by both Johnston and the Barrows Collection). Of these, the Barrows Collection detected 102 sets of fiscal terms (the number detected by both sources). Thus, we can say that the Barrows Collection has a detection probability of 103 / 143 = 72%. The Barrows Collection detected 235 separate sets of fiscal terms over the period (133 detected by the Barrows Collection + 102 detected by both). Thus, we can say that the Johnston database has a detection probability of 102 / 235 = 43%. Thus, the Johnston database is less likely to have captured fiscal terms. However, this does not take into account the quality of information in each detection event. The Johnston database tended to provide less missing data within each set of fiscal terms published in the database. The combined non-detection rate is the product of the two non-detection rates: (1-.72) * (1- .43) = 16%. This suggests that the two sources combined detected 84% of the total number of fiscal terms, a fairly high detection rate given the difficulty in securing information on fiscal terms within the oil industry.[33]

We might be concerned that the detection rate is higher for some subsamples of the data than others. In particular, the detection rate might be higher among democracies than autocracies. The combined non-detection rate among democracies is 6.5%[34] and the combined non-detection rate among autocracies is 16%.[35] Thus, the detection rate is about 11% higher among democracies compared to autocracies. However, when we look at this in a regression framework, we can see that more democratic states are no more likely to be included in the database (Table 3). Table 3 examines variables that could plausibly related to a country's tendency to release information on fiscal terms, including regime type, per capita income (richer states could have more capable bureaucracies to manage and release data), and indicators of the size and history of oil production (OPEC membership, years since first oil production, size of oil reserves). Poorer states are less

[33]This method is fully explained and developed in Hendrix and Salehyan (2015) and is applied to the reporting of conflict-events in Africa.

[34]Among states with a Polity score greater than zero, the Johnston database detects 47 events (6 detected by Johnston + 41 detected by both). Of these the Barrows Collection detected 41, thus the Barrows Collections' detection probability among democracies is 41 / 47 = 87%. The Barrows Collection detected 82 events (41 detected by the Barrows Collection + 41 detected by both), giving the Johnston database a detection probability of 50%.

[35]Among states with a Polity score of less than or equal to zero, the Johnston database detects 93 events (34 detected by Johnston + 59 detected by both). Of these the Barrows Collection detected 59, thus the Barrows Collections' detection probability among autocracies is 59 / 93 = 63%. The Barrows Collection detected 141 events (82 detected by the Barrows Collection + 59 detected by both), giving the Johnston database a detection probability of 42%.

likely to be included in the Barrows Collection and to be included in both databases. States that have been producing oil for longer periods of time are more likely to be included by the Barrows Collection, as are states with smaller oil reserves and states that are not members of OPEC.

Table 2.3: Regression Analysis of Missing Data, 1980-2000

	Johnston database	Barrows Collection	Included in Both
Polity	0.003	-0.019	-0.003
	(0.023)	(0.040)	(0.024)
GDP per capita	0.130	0.289	0.266*
	(0.130)	(0.181)	(0.132)
OPEC	0.155	-1.457**	-0.401
	(0.352)	(0.530)	(0.373)
Years Since First Production	-0.007	0.026**	0.002
	(0.004)	(0.009)	(0.004)
Oil Reserves (billions barrels)	0.005	-0.013+	-0.006
	(0.007)	(0.007)	(0.007)
N	230	230	230

*Note: *** p-value < 0.01, ** p-value <0.05, * p-value < 0.10. Models report coefficients from logit models with standard errors in parentheses.*

One would expect that rates of missing data would look substantially different if we were to rely on self-reported information on oil contracts. However, by relying on two independent third-party data sources, I am able to gather information on fiscal terms with a low level of overall missingness and with relatively little bias. Although this analysis reassures us that the rate of missingness and the potential bias caused by missingness are lower than would be expected given the heavy secrecy surrounding oil contracts, the issue of missingness is by no means resolved. The dataset does not contain the universe of oil contracts, and it is difficult to assess how large the potential universe of contracts even is. Future iterations of this work will match the collected data onto a list of all potential contracts that may have been signed in the oil sector (to be defined by the number of global oil fields) to better assess rates of missingness and any bias that may result.

2.5.2 *Measurement: Dependent Variables*

Even once we've collected the fiscal terms from oil-producing countries, including all the fiscal terms defined within oil contracts and national laws, we still do not know whether the government secured a high take or not or how front-loaded the tax regime is. In order to assess these key aspects of the fiscal environment, it is necessary to perform cash flow analysis to assess the impact of various fiscal terms. However, to analyze the fiscal terms, one must make assumptions about both the geology, including field size and costs of extraction, and the profitability, including the price of oil, of the field. Not only are these quantities unobservable *ex post* due to lack of field-by-field data on project economics and field size, but these quantities are unknowable *ex ante* to the contract signatories.

Therefore, I subject the sets of fiscal terms to a set of 36 different scenarios, based on differences in field size and field profitability.[36] The scenarios are defined as follows. First, I establish four different scenarios for possible field sizes: a small field size of 50 million barrels, a medium-small field size of 100 million barrels, a medium-large field size of 500 million barrels, and a large field size of 1 billion barrels. Within each field size scenario, I consider 9 different field profitability scenarios, in which costs as a percentage of revenues are 0%, 7%, 15%, 30%, 45%, 60%, 75%, 90%, and 100%.[37] So, a scenario in which costs represent only 7% of revenues, for example, considers either the possibility that oil prices are exceptionally high or that costs of extraction are exceptionally low. One of the first things to note is that fiscal terms are frequently not "commercial" under some of the scenarios. I define whether or not fiscal terms are commercial–meaning whether the MNC would invest at all given the expected returns–based solely on whether the net present value of the IOC's cash flow is greater than zero. In other words, I use the most permissive definition possible. In fact, the fiscal terms define what constitutes a commercial discovery: if the government set the tax rate at zero, then small fields with high costs of extraction could be

[36] All scenarios make the same assumptions about decline rate and field life, described in the Appendix.

[37] The profitability scenarios are not equally likely across field sizes. Large fields are much more likely to have low costs of extraction as compared to smaller fields. In fact, the differences in field size only matter for the analysis in so far as fiscal terms are explicitly linked to production levels.

produced and still yield a positive rate of return, yet as the tax rate rises, fields must be larger and more profitable in order to yield the same rate of return for the investor.

For the majority of the paper, I utilize the results of what I call the "most-likely" scenario for each country, based on the estimated costs of extraction[38] and the price of oil at the time of signing the contract or enacting the law. However, I probe the results across the scenarios as well.

First, government take is "the government's share of economic profits from almost all income sources, including bonuses, royalties, profit oil, taxes, and government working interest" (Johnston 2007: p. 56). I include the NOCs' share of profits as a part of government take. Thus, I do not make a distinction between revenues accruing to the central government versus to NOCs. For countries that have fully nationalized their oil industry, like Saudi Arabia, I assume that the government take is 100%.

Government take calculations are sensitive to modeling assumptions. As a simple example, a fiscal regime that consisted of just a 20% royalty would yield different government take statistics depending on the profitability of the oil field. If costs were 40% of gross revenues, then the royalty would secure the government one-third of the rents from oil production, whereas if costs were merely 20% of gross revenues, then the same royalty would only secure the government one-quarter of the rents from oil production. Making calculations more sensitive is the fact that fiscal terms often vary depending on field size or project profitability. Holding field size constant, I calculate government take based on the estimated costs of extraction and the real oil prices when terms are initially set (the "most-likely" scenario).[39]

[38] Unfortunately, costs of extraction (or, "lift costs") are not publicly available in cross-national time-series format. As a proxy for costs of extraction, I estimate them using data from a Goldman Sachs report on the largest 125 oil development projects (Waghorn et al. 2006). For countries for which there is no data at all, I assume that costs of extraction are the same as for neighboring states based on the assumption that neighboring states will share similar geographies. I thank Jeff Colgan for sharing this data.

[39] Field size primarily influences government take through its impact on field profitability: economies of scale tend to make larger fields more profitable. Thus, different assumptions about field sizes do not have much effect on government take calculations.

Figure 2.3: Distribution of Government Take

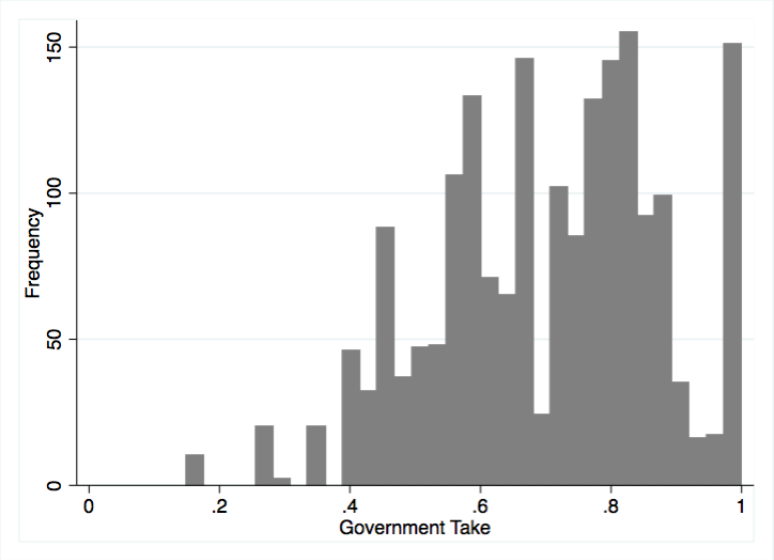


Figure 3 shows the distribution of government take for the 779 separate sets of fiscal terms in the sample. Government take is calculated using the "most-likely" scenario for oil prices and costs of extraction for each contract.

To assess the front-loadedness of the fiscal terms, or the extent to which the government's share of revenues is accrued early in the life of the contract, I look at the ratio between the government's cash flows in year 1 and the government's cash flows in the peak year of field production. The government's cash flow in year 1 includes signatory bonuses, any taxes paid by the MNC that are paid irrespective of whether the field is profitable yet (e.g. royalties based on a percent of a production, profit oil splits paid because there are cost recovery limits), and government cash outflow due to responsibilities for up-front capital costs. As such, it takes into account the effects of governments' equity stakes, accelerated depreciation schedules, cost recovery limits, royalties, and bonuses on contract front-loadedness. A higher ratio means the contract is more front-loaded.[40]

[40]This measure is preferable to looking at the Effective Royalty Rate proposed in Johnston (2007) because it takes into account the effects of government participation, bonuses, and depreciation schedules in addition to cost recovery limits and royalties.

Figure 2.4: Distribution of Front-Loadedness

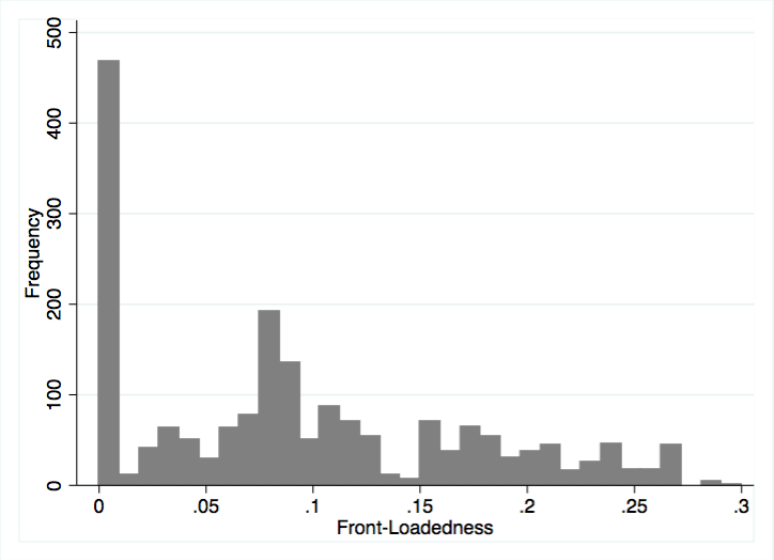


Figure 4 shows the distribution of front-loadedness for the 779 separate sets of fiscal terms in the sample. Front-loadedness is calculated using the "most-likely" scenario for oil prices and costs of extraction for each contract and is the ratio between government cash flow in year 1 and government cash flow during the peak year of production. Negative ratios result if the government's cash flow is negative in year one (because of responsibility in year 1 for exploration costs).

To assess government exposure to risk, I examine the contractual terms regarding government participation. I look specifically at the extent to which the form of participation exposes governments to project risk. When contracts arrange for governments to carry a working interest through the exploration phase and they only pay their share of exploration costs if there is a commercial discovery, I consider this as a scenario of zero risk for the government. 48% of the 779 fiscal terms in the sample contain a government ownership stake; of these, 39% involve the government taking on exploration risk.

I measure investment guarantees using the minimum work clauses in contracts. I look at the total of all of the minimum work obligations in the contract. I then divide the total minimum work obligations by annual oil production in the country to account for the fact that $1 million investment is more substantial in a minor producer compared to a major producer.

I measure the progressive of the fiscal regime using the scenario analysis. I examine the degree of progressivity by taking the difference between the government take statistic for the scenario

in which costs are 45% of revenues and the government take statistic in which costs are 0% of revenues (a windfall profit environment). When the less profitable scenario yields a government take statistic that is greater than the more profitable scenario, this indicates a regressive regime. When the more profitable scenario yields higher government take, this indicates a more progressive taxation regime.

2.5.3 *Measurement: Independent Variables*

To measure regime type, I use Polity IV. I measure time horizons within democracies and autocracies separately. I differentiate between democracies and autocracies using Polity scores. Countries with a polity score greater than four are included in the analysis of democracies, and countries with a polity score of less than or equal to four are included in the analysis of autocracies. To measure a democratic government's time horizon, I focus on the institutionalization of the ruling party. I follow Blake (2013) and Simmons (2008) in using the age of the ruling party as a proxy for party institutionalization. Data on the age of the ruling party[41] is from the World Bank's Database of Political Institutions (Beck et al. 2001).

To measure an autocratic regime's time horizon, I follow Blake (2013) and Wright (2008) in using the predicted probability of autocratic regime failure as a proxy for the leader's time horizons. These estimates come from a model of autocratic survival, as defined in Wright (2008). The model includes (log) GDP, lagged growth (t-1 and t-2), Islam, civil war, region controls, and information on autocratic institutions from Geddes et al. (2014). The model is reported in the Appendix.

2.5.4 *Controls*

All models control for factors beyond political regime and time horizons which might enhance governments' ability to secure better deals with IOCs. First, I measure the quality of the oil endowment, or, the attractiveness of the HC as an investment site in the oil sector. I measure the quality of the resource endowment based on the history of oil production in the country and the

[41] The age of the largest party in the government (the president's party in presidential systems, otherwise the largest party in the governing coalition).

country's geology. Countries with longer histories of commercial oil extraction[42] should have less uncertainty around the size and location of reserves, enabling less risky investments. I measure the geology of the country's endowment based on the costs of oil extraction[43], whether the country is a member of OPEC (countries which tend to have lower costs of extraction and larger oil fields), and the size of proved reserves in the country.[44] Because the size of oil reserves is only available 1980-present, I only include this measure in some of the models so as not to truncate the sample in all of the models. I further control for international oil prices (2013 USD).[45]

I additionally control for factors which could shape credibility of commitments beyond regime type. I control, first, for constraints on the executive. I measure constraints on the executive using the Henisz's Political Constraint Index, which measures the ability of political structures to support credible policy commitments. It uses information on the number of independent branches of government, the degree of alignment across the branches of government, and the degree of preference alignment within each legislative branch of government. For the index, each additional veto has a positive but diminishing effect on the level of constraints on policy change.[46] Finally, I further measure the credibility of a country's commitments to investors within the oil sector specifically by looking at the history of expropriations and contract renegotiations within the country. I use two main sources to gather information on the history of expropriations. First, I utilize data on nationalization events from Guriev et al. (2011). I supplement this with data from my own data collection effort. I code national laws that unilaterally increase government ownership in oil projects or increase tax on oil development as "expropriation events." For each year, I take the cumulative sum of the total number of past expropriation events.

[42]Data from Mahdavi (2014).
[43]Data from Waghorn et al. (2006). Discussed in footnote 38.
[44]Data from Ross (2012).
[45]Data from *BP Statistical Review of World Energy*.
[46]I use the POLCONIII_2002 index.

2.6 Results

2.6.1 *Political Regimes*

When do countries get better deals in their negotiations with IOCs, and how do political institutions influence bargaining outcomes? Table 4 examines the determinants of government take, front-loadedness, investment guarantees, government risk exposure, and progressiveness of the fiscal regime. I find that more democratic countries receive lower government take and less front-loadedness of payments, despite the idea that they should be able to make more credible commitments to investors. A one-point change on the Polity scale is associated with a 1.2 percentage point decrease in government take. A 14-point change in the Polity scale, the difference between being a fairly autocratic state (with a score, say, of -7) and a fairly democratic state (with a score of +7) is associated with a one-half of a standard deviation decrease in the level of government take. This is substantial when one considers that, for a minor oil producer, like Cote d'Ivoire used as an example earlier in the paper, the difference of a single percentage point in government take is equivalent to nearly $7 million in foregone tax revenues annually if oil rents are at $50 a barrel.[47] For a medium-sized oil field of 100 million barrels with $50 in rents per barrel, a single percentage point difference in government take amounts to a $71 million difference in what the government can expect to collect over the life of the field and a half a standard deviation difference in government take (17 percentage points) would amount to a $1.2 billion difference over the life of the field. This is only for a single oil field. The foregone take would be multiplied the more oil that is being developed within the country.

Not only do more democratic countries receive lower levels of government take, but they also receive their shares less quickly. This is exactly the opposite of what would be desirable from a policy perspective. To the extent that it is beneficial for citizens for governments to sacrifice take in exchange for more front-loadedness, we would want more democratic countries to make this trade-off, assuming that more democratic countries are more likely to invest in physical and

[47] 37,000 barrels of oil per day * 365 * $50 * 1% = $6.7 M

Table 2.4: Regression Analysis, Regime Type and Fiscal Terms, 1974-2004

	Gov't Take *(% profits) * 100*		Front-Loadedness *(P(1st) / P(8th))*100*		Investment *(min. inv. / oil prod.)*		Flexibility *(take high profit - take low profit) * 100*		Risk *(% up-front exp. costs) * 100*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
L.Polity	-1.173**	-1.0653**	-0.276+	-0.263+	0.111	-0.071	-0.040	0.033	-1.166+	-1.128+
	(0.241)	(0.240)	(0.163)	(0.156)	(0.311)	(0.316)	(0.269)	(0.294)	(0.593)	(0.587)
Political constraints	5.244	0.547	5.899	5.246	-2.330	7.631	-17.91+	-19.30*	11.52	14.54
	(6.961)	(6.808)	(4.378)	(4.630)	(7.701)	(10.12)	(9.251)	(9.067)	(14.27)	(13.55)
History of exp.	-1.282*	-1.581**	0.652	0.488	-0.111	0.425	2.404**	2.311*	-1.495	-1.410
	(0.551)	(0.563)	(0.446)	(0.452)	(0.817)	(0.950)	(0.849)	(0.919)	(1.583)	(1.678)
Yrs. since oil prod.	0.152**	0.127**	0.003	-0.005	-0.168	-0.194	0.006	-0.021	0.279*	0.247*
	(0.048)	(0.045)	(0.020)	(0.021)	(0.142)	(0.165)	(0.049)	(0.056)	(0.106)	(0.097)
Costs of extraction	-0.364	-0.322	-0.398+	-0.4306*	-0.196	-0.220	-0.852+	-0.891+	-0.880	-0.820
	(0.528)	(0.530)	(0.202)	(0.2095)	(0.563)	(0.656)	(0.501)	(0.494)	(1.137)	(1.135)
Oil reserves (log)		0.555+		0.115		-0.742		0.075		-0.043
		(0.281)		(0.096)		(0.684)		(0.283)		(0.683)
OPEC	8.292**	8.172*	-6.628**	-6.901**	-0.482	1.836	-2.345	-3.646	-0.336	3.064
	(3.031)	(3.106)	(2.236)	(2.237)	(3.977)	(4.361)	(4.626)	(5.064)	(6.944)	(7.331)
Oil price (2013 $)	-0.004	-0.013	0.051*	0.054*	0.071	0.145	-0.042	-0.041	0.072	0.077
	(0.034)	(0.039)	(0.022)	(0.024)	(0.101)	(0.164)	(0.036)	(0.039)	(0.063)	(0.063)
GDP per capita (log)	1.997	1.347	-0.549	-0.422	-5.129	-6.707	-1.338	-1.540	3.087	2.088
	(1.297)	(1.282)	(0.677)	(0.703)	(4.504)	(5.972)	(1.296)	(1.567)	(2.972)	(3.110)
N	1,095	933	1,095	932	249	211	1,024	862	1,095	933

*Note: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. For each contractual term, the term is calculated using the "most-likely" scenario for oil prices and costs of extraction for each contract. Standard errors clustered by country in parentheses.*

human capital compared to more autocratic ones.

Why would democracies tax oil production less than autocracies? This cuts against the idea that democracies use their enhanced credibility of commitments to bargain for higher tax levels. One explanation is that more democratic countries are pursuing a different strategy with regards to the oil industry than more autocratic countries. More democratic countries may be securing lower government take, but they by securing lower take they may be ensuring that oil fields are worth developing commercially across a wider range of size and profitability scenarios. Figure 5 plots the coefficients from linear probability models for the effect of Polity score on whether or not fiscal terms in place in the country at the time are commercial for the most-likely scenario (i.e. they yield a positive net present value for the investor). All models include the same controls as Table 4, Model 1.

Very few sets of fiscal terms are commercial in the low profitability scenarios. When costs are 75% of revenues, for example, only 9% of the observations in the sample would yield a positive net present value for the investor. However, when we look at the global average profitability scenario–with costs at 30% of revenues–and a fairly low but realistic profitability scenario–with costs at 45% of revenues–more democratic countries are indeed more likely to set more commercial fiscal terms. At higher levels of profitability, there is no difference between regime types in whether or not fiscal terms are commercial. Thus, it appears that democracies are developing fields with higher costs of extraction and in lower oil price scenarios that simply would not be developed with higher fiscal terms.

Figure 2.5: Effect of Polity on Commerciality of Fiscal Terms Across Profitability Scenarios, Coefficients and 90% Confidence Intervals

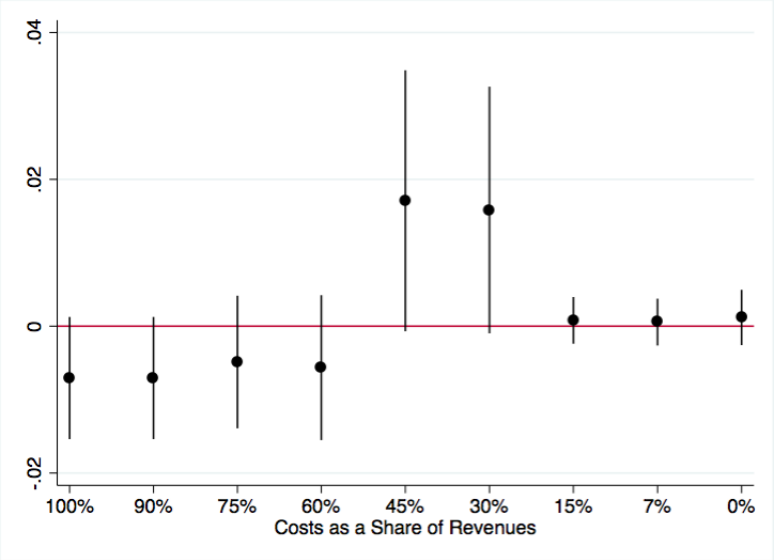


Figure 5 graphs the coefficients and 90% confidence intervals from linear probability models for the effect of Polity score on whether or not the fiscal terms in place in the country at the time are commercial or not (yield a positive net present value for MNCs) at various levels of potential field profitability, All models include the same controls and variables as Table 4, Model 1. Standard errors are clustered by country.

They are doing so at lower levels of risk exposure compared to autocracies. There are at least two explanations. First, if democratic governments have more ability to make credible commitments, then they do not need to take on as much risk in order to secure equal levels of take. In this view, risk is a way to resolve credible commitment problems: states with less secure property rights can reduce their own incentives to expropriate down the line by taking an equity stake through a joint venture (Jensen and Johnston 2011; Henisz 2002). Autocracies with less secure property rights take on more risk as a way of gaining more government take that they would not otherwise be able to get in bargaining with IOCs.

Second, it is possible that revenues collected through the NOCs that take the equity stakes in joint venture projects are more valuable to autocratic governments. Collecting revenues through national oil companies could offer benefits to government officials that standard royalties and taxes don't. National oil companies can allow politicians to collect rents "off-budget," as the budgets of state-owned oil companies are frequently shielded from legislative and public oversight. If

revenues collected through state-owned oil companies are more easily concealed than revenues collected through royalties and income taxes, then they should be particularly valuable to predatory politicians.[48]

Regime type plays a complex role in shaping relationships with IOCs. More democratic countries set lower overall tax rates, but this also means that they are making marginal oil fields commercial. Thus, democratic countries may be better at replacing reserves over time, particularly as costs of extraction tend to grow over the life of oil production. Indeed, if you consider countries that are democratic in 1985 (with a Polity score >= 4), on average, they increase their reserves-to-production ratio by 13 years between 1985 and 2000. The reserves-to-production ratio is simply the amount of proved oil reserves over the amount of oil produced in a given year and represents the length of time that reserves would last if they were produced at the current rate without replenishment.[49] The reserves-to-production ratio is inversely related to rate of extraction, meaning that countries with higher reserves-to-production ratio are converting natural capital into financial capital at slower rates, and positively related to reserves, meaning that countries that are better able to replace reserves will maintain higher ratios. In other words, it is a measure of savings in the ground. Autocratic countries, by contrast, during the same period on average decreased their reserves-to-production ratios by 5 years.[50] Thus, it appears that rather than using a position of increased bargaining power to secure better terms in each individual deal vis-a-vis IOCs, more democratic countries are lowering fiscal terms to produce fields under a broader range of profitability scenarios and are doing so with lower risk exposure compared to more autocratic countries.

These findings hold in fixed effects models, enhancing confidence that the finding is not the result of reverse causality (Table 5). Changes in levels of democracy within countries are also associated with lower tax rates, less front-loadedness of taxation regimes, and less government exposure to project risk.

Control variables also yield interesting findings. Countries that are more attractive sites for

[48] Although autocratic governments likely face little oversight for normal budgets as well.

[49] They are often falsely understood as the amount of time before a country runs out of oil. However, this does not take into account the fact that countries can replenish reserves over time by making new investments and discoveries.

[50] Author's calculations using data on oil reserves from Ross (2012).

Table 2.5: Fixed Effects Models, Regime Type and Fiscal Terms, 1974-2004

	Gov't Take *(% profits) * 100*	Front-Loadedness *(P(1st) / P(8th))*100*	Investment *(min. inv. / oil prod.)*	Flexibility *(take high scen. - take low scen.) * 100*	Risk *(% up-front exp. costs) * 100*
	(1)	(2)	(3)	(4)	(5)
L.Polity	-0.916**	-0.193**	0.043	-0.203	-1.152**
	(0.123)	(0.069)	(1.392)	(0.137)	(0.193)
Political constraints	-3.680	1.958	1.025	-11.67**	-6.986
	(3.029)	(1.701)	(35.45)	(3.410)	(4.765)
History of exp.	3.656**	0.702+	-7.819	3.141**	9.727**
	(0.690)	(0.388)	(11.71)	(0.783)	(1.085)
Yrs. since oil prod.	-0.136*	0.046	0.985	0.113+	-0.395**
	(0.058)	(0.032)	(0.794)	(0.064)	(0.090)
Costs of extraction	-1.188**	0.562**	-1.737	0.188	-4.361**
	(0.322	(0.181)	(2.753)	(0.457)	(0.507)
OPEC	1.604	0.291	8.114	18.71**	-2.175
	(2.667)	(1.498)	(35.27)	(3.354)	(4.195)
Oil price (2013 $)	-0.063**	0.054*	0.239	-0.006	-0.082**
	(0.018)	(0.010)	(0.209)	(0.002)	(0.028)
GDP per capita (log)	3.503**	-1.674**	-16.31	-1.759	6.821**
	(1.045)	(0.587)	(13.18)	(1.157)	(1.643)
N	1,095	1,095	249	1,024	1,095

*Note: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. For each contractual term, the term is calculated using the "most-likely" scenario for oil prices and costs of extraction for each contract. All models include country fixed effects. Standard errors in parentheses.*

oil investments–as indicated by the history of oil exploration and production in the country and the size of proved oil reserves–tend to receive higher levels of government take. Surprisingly, these factors, all else equal, do not seem to help countries to secure more front-loaded contracts, higher investment guarantees, or more progressive taxation regimes. For front-loadedness, instead of history of exploration or size of reserves, what matters is costs of extraction.[51] This makes sense: IOCs are going to demand accelerated depreciation schedules and high cost recovery limits in areas where the costs of extraction are especially high. A similar logic explains why oil prices are positively related to countries' ability to secure more up-front revenues: high oil prices help companies to recover costs more quickly, reducing the cost to companies of front-loaded fiscal regimes.

Credibility of commitments–apart from regime type–also play a role in shaping the design of fiscal regimes, but not always in the expected direction. As expected, countries with greater incidences of past expropriations and contract renegotiations secure lower levels of government take.[52] For a medium-sized oil field of 100 million barrels with $50 in rents per barrel, a country with an additional instance of past expropriation would secure $81 million less in revenues over the life of the oil field. It is worth noting that the fiscal terms in my dataset incorporates information on both contracts and renegotiations / expropriations. So, if a country that has expropriated in the past signs a new contract with low fiscal terms in order to attract investment but later unilaterally increases the tax rate, both of these events are incorporated into the annual dataset on fiscal regime design. This means that countries with longer histories of expropriation are not just negotiating contracts with lower take and then making up for this later through renegotiations. Every additional expropriation means lower tax rates in the future, even allowing for contract renegotiations. Interestingly, countries with histories of expropriation do design more progressive fiscal systems. This could indicate

[51] Because costs of extraction are measured as a constant within countries, these coefficients are not terribly meaningful in the fixed effects models. They vary only in countries that shift from developing onshore to offshore resources over time (or vice versa).

[52] The effect is the opposite in the fixed effects models: with one additional instance of past expropriation associated with *increased* government take. This make sense: an act of expropriation should immediately increase government take. However, the fact that countries with higher reputations for extraction cannot set tax rates at levels comparable to countries with reputations for protection of property rights is of more theoretical interest (the between-country effect).

learning: countries that have renegotiated contracts in the past because they failed to receive a fair share of profits in a windfall environment may design more progressive regimes going forward.

Surprisingly, none of the measures of attractiveness of the investment site or of the investment climate help to explain the minimum levels of investment secured within contracts. This is true even if we examine investment guarantees measured in different ways (e.g. total minimum investment guarantees, minimum investment guarantees during the first 5 years of the contract).

2.6.2 *Time Horizons*

Table 6 examines the effects of government time horizons on government take, front-loadedness, investment guarantees, progressivity of taxation, and government risk exposure within democracies and autocracies separately. Among democracies, time horizons play a significant role in shaping how governments design fiscal regimes. As time horizons extend, democratic governments are less likely to front-load contracts and collect higher levels of government take. They are also more likely to design progressive fiscal regimes. All of these factors are exactly what is suggested by the theory: governments with longer time horizons are more willing to give up some revenues now in exchange for greater revenues in the long term. More forward-thinking governments are also better at ensuring that their interests are secured should windfall profit scenarios arise (reducing the need for contract renegotiations). However, time horizons do not seem to be playing a role in securing higher investment guarantees. Among autocracies, differences in time horizons do not have a significant direct effect on tax regimes.

In next examine how exogenous differences in oil prices condition the effects of time horizons on outcomes. In doing so, I assume that oil prices make oil endowments more attractive for foreign investment, conferring bargaining power to HCs. This question essentially asks, at different levels of bargaining power, how do time horizons differentially affect policy choices about how to govern the resource sector? Most interestingly, how do governments with longer vs. shorter time horizons react differently to the opportunities provided by high oil prices? Do governments use greater bargaining power to grab up-front revenues like signatory bonuses? Or, do they use it to renegotiate

Table 2.6: Regression Analysis, Time Horizons and Fiscal Terms, 1974-2004

	Gov't Take *(% profits) * 100*		Front-Loadedness *(P(1st) / P(8th))*100*		Investment *(min. inv. / oil prod.)*		Flexibility *(take high profit - take low profit) * 100*		Risk *(% up-front exp. costs) * 100*	
	Dem	Aut	Dem	Aut	Dem	Aut	Dem	Aut	Dem	Aut
L.Party Age	0.122*		-0.056*		-0.005		0.080*		0.063	
	(0.036)		(0.020)		(0.004)		(0.031)		(0.072)	
Prob. Failure		-0.584		-0.118		0.141		-0.475		-0.237
		(0.398)		(0.229)		(0.508)		(0.317)		(0.788)
N	245	671	245	671	52	157	233	604	245	671

*Note: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. For each contractual term, the term is calculated using the "most-likely" scenario for oil prices and costs of extraction for each contract. Standard errors clustered by country in parentheses. All models control for constraints on executive, history of expropriations, years since first commercial oil production, extraction costs per barrel, (log) GDP per capita, OPEC membership, and oil price as controls.*

Table 2.7: Interactions Between Party Age and Oil Prices, 1974-2004

	Gov't Take *(% profits) * 100*	Front-Loadedness *(P(1st) / P(8th))*100*	Investment *(min. inv. / oil prod.)*	Flexibility *(take high profit - take low profit) * 100*	Risk *(% up-front exp. costs) * 100*
	(1)	(2)	(3)	(4)	(5)
L.Party Age * Oil Price	-0.001	-0.000	0.001**	0.000	-0.000
	(0.001)	(0.000)	(0.000)	(0.000)	(0.001)
L.Party Age	0.146**	-0.050*	-0.029**	0.064	0.076
	(0.048)	(0.019)	(0.008)	(0.043)	(0.111)
Oil Price (2013 $)	-0.071	0.138**	-0.020	-0.039	-0.099
	(0.095)	(0.040)	(0.031)	(0.112)	(0.097)
N	245	245	54	233	245

*Note: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. For each contractual term, the term is calculated using the "most-likely" scenario for oil prices and costs of extraction for each contract. Standard errors clustered by country in parentheses. All models control for constraints on executive, history of expropriations, years since first commercial oil production, extraction costs per barrel, (log) GDP per capita, and OPEC membership. The sample includes only those countries with Polity scores greater than or equal to 4.*

Table 2.8: Interactions Between Probability of Autocratic Regime Failure and Oil Prices, 1974-2004

	Gov't Take *(% profits) * 100*	Front-Loadedness *(P(1st) / P(8th))*100*	Investment *(min. inv. / oil prod.)*	Flexibility *(take high profit - take low profit) * 100*	Risk *(% up-front exp. costs) * 100*
	(1)	(2)	(3)	(4)	(5)
Prob. Failure * Oil Price	-1.082	0.878+	1.084	-0.583	-3.819+
	(1.003)	(0.471)	(1.831)	(0.977)	(2.036)
Prob. Failure	5.549	-62.05	-51.93	-11.75	242.5
	(85.08)	(37.15)	(138.8)	(76.10)	(157.1)
Oil Price (2013 $)	0.048	0.020	0.088	-0.019	0.282*
	(0.036)	(0.028)	(0.170)	(0.053)	(0.113)
N	671	671	157	604	671

*Note: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. For each contractual term, the term is calculated using the "most-likely" scenario for oil prices and costs of extraction for each contract. Standard errors clustered by country in parentheses. All models control for constraints on executive, history of expropriations, years since first commercial oil production, extraction costs per barrel, (log) GDP per capita, and OPEC membership. The sample includes only those countries with Polity scores less than 4.*

higher and more progressive tax rates? Secure higher investment guarantees?

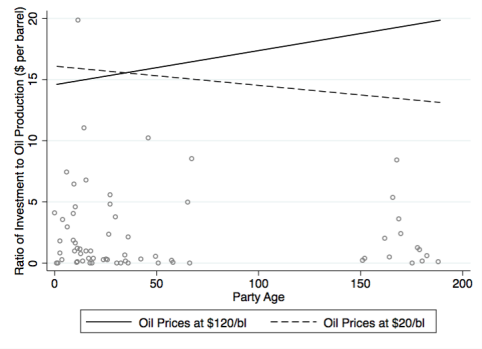
Tables 6 and 7 report the interaction terms (as well as the constituent terms) between measures of time horizons and oil prices within democracies and autocracies respectively. Among democracies, governments with longer time horizons tend to sign new contracts seeking higher investment guarantees when oil prices are high. Governments with longer time horizons within democracies thus use their increased bargaining power to further induce foreign investment in exploration and production. This finding is in line with the overall characterization of democracies presented above. Democracies tend to use bargaining power as a means to attract investment and develop the size of the oil reserves over the long-term. This is especially true among those democracies with more established political parties that have higher prospects of returning to office in the future to enjoy increased access to tax collections from a larger resource base. Figure 6a further elucidates the effect by graphing the effect of party age on investment guarantees when oil prices are high vs. low. When oil prices are at $120/barrel, countries with the least institutionalized political parties garner $5 less per barrel in investment as compared to countries with the most institutionalized parties, a 25% difference.

Among, autocracies, governments with shorter time horizons (higher predicted probabilities of failure) tend to increase the front-loadedness of fiscal regimes and reduce their exposure to project risk when oil prices are high. Reducing their exposure to project risk likely means that governments are selling off nationally-owned oil fields and stakes in oil development projects, valuable ways of filling government coffers with cash in the present. More directly, autocratic governments with short time horizons are increasing the front-loadedness of their tax regimes with oil prices. This means that autocratic leaders at risk of losing office–a set of leaders most likely to consume oil revenues to shore up their regime and to fund repression and least likely to invest in physical and human capital–are not only wasting irreplaceable oil revenues, they are also wasting the valuable opportunity that high oil price environments provide to increase their government's overall claim to oil rents over the long term or to increase foreign investment levels. A government with a 20% predicted probability of failure compared to a government with a near 0% predicted probability of

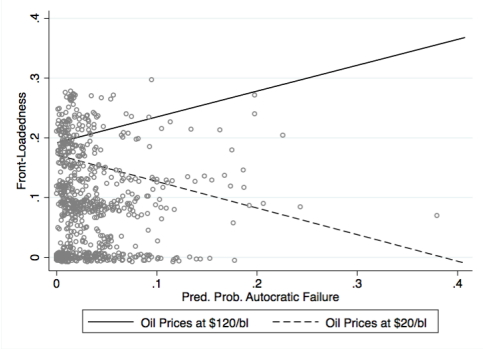
failure increases the front-loadedness of the fiscal regime by 30%, holding oil prices constant at $120/barrel (Figure 6b).

Figure 2.6: Interactions Between Time Horizons and Oil Prices

(a) Effect of Party Age at High vs. Low Oil Prices



(b) Effect of Probability of Autocratic Regime Failure at High vs. Low Oil Prices



Figures 6a-b graph the effect of party age and predicted probability of autocratic regime failure on investment guarantees per barrel of oil produced ($ per barrel), front-loadedness of tax regimes (ratio between revenues in year 1 to year 8), and government risk exposure (the share of total exploration costs that the government must supply up-front) when prices of oil are $20 per barrel and $120 respectively.

2.6.3 *What do the findings mean for which countries are better managing resource endowments?*

What do these results as a whole tell us about the kinds of countries that most optimally manage their resource wealth? This question is difficult to answer, not least because it is hard to define what we mean by optimal resource governance. Overall, I find that democracies and autocracies pursue distinct strategies. More democratic countries, that should be better able to make credible commitments not to expropriate from multinational companies and thus gain better *ex ante* deals, in fact set lower tax rates and collect fewer up-front revenues, enabling them to assume lower levels of project risk and to increase the commercial viability of marginal fields. These findings hold whether we are considering differences between or within countries in levels of democracy. Democracies with more institutionalized political parties (longer time horizons) tend to reduce their claim to early revenues even more, but this does enable them to collect higher tax rates compared to democracies with less institutionalized political parties. Longer time horizons also

motivate democratic politicians to design more flexible fiscal regimes, planning for future potential windfall profit scenarios and enhancing the government's claim to these windfalls. They also seize the opportunity offered by high oil price environments to secure higher levels of investment from foreign companies.

More autocratic countries take a decidedly different approach to natural resource management. They tend to ratchet up tax rates, frequently at the expense of putting the government in a risk-bearing position, at the expense of the commerciality of smaller fields. Under these conditions, only the largest oil fields with low costs of extraction would even be developed; others would be considered unviable. Time horizons matter less in distinguishing behavior among autocracies as compared to among democracies. However, autocratic politicians facing higher predicted probabilities of regime failure do use high oil price environments to opportunistically grab early revenues. This is particularly short-sighted because front-loaded taxation regimes are regressive, and, by focusing on regressive taxation methods that enable quick revenue collection, governments are missing the opportunity to secure windfall profits through progressive taxation.

I have characterized the strategy of democracies by saying that democracies are taking a long view with regards to their oil sector, enacting policies that can increase the overall size of their reserves over time. This may be true, but it is hard to pin down whether this represents "optimal" resource governance. On the one hand, promoting investment enhances growth in the sector and replenishes reserves, building up countries' savings in the ground and passing along a larger natural resource endowment to the next generation. This seems better than the typical characterization of resource-rich governments plundering nationally-owned resources for private gain, consuming resource capital more quickly than it can be built up, making countries poorer over time (Heal 2007).

However, in doing so, democracies may be foregoing opportunities to capture additional shares of oil rents that are key contributors to national wealth in many developing countries. It is not obvious whether setting the highest tax rates possible on a shrinking resource base–i.e. claiming the maximum possible share on every unit of oil that comes out of the ground–will yield more

or less income over time than setting lower tax rates on a (probabilistically) growing resource base. Democracies may even have higher socially optimal tax rates on the extractives industry compared to autocracies as they can better commit to investing oil income in physical and human capital, harnessing resource revenues for growth. What constitutes good resource management is a matter for continued discussion and debate and depends on political context. This paper shows that political institutions shape resource governance strategies.

However, the findings do offer some policy insights into how countries can improve natural resource governance. The fact that there are tradeoffs among different elements of oil contracts suggests that governments can improve how they fare along one dimension, even if they have to make sacrifices along another. So, for example, a government willing and able to assume some project risk can secure higher claims to profits and automatically ensure that their claim to rents increases as rents rise. Progressivity in fiscal regimes represents a unique opportunity to improve how governments fare in bargaining outcomes as there do not seem to be tradeoffs between designing a progressive fiscal regime, which can capture windfall profits, and other dimensions of contract value. The costs to designing a progressive fiscal regime should be especially low in the current low oil price environment, but doing so now would ensure that government's better secure their interests should market conditions change once again. It could diminish the incentives for future renegotiations, which cost countries in the long run.

2.7 Conclusions

Using an original dataset on historically confidential oil contracts and pieces of hydrocarbons legislation, this paper tests how political institutions shapes natural resource governance. I measure resource governance by looking at five dimensions of the financial arrangements between host countries and international oil companies: the division of overall profits between parties, the extent to which the government secures its share of profits in early years, the minimum investment guarantees that oil companies make to host country governments, the degree to which the government is exposed to oil exploration risks, and the ability of the taxation regime to capture windfall

profits. I describe theoretically and show empirically that governments face trade-offs along these dimensions when designing fiscal regimes to govern oil extraction: higher taxes, for example, are associated with lower investment guarantees. Although these relationships are intuitive, we are typically reliant on highly aggregated cross-national data to assess them empirically. Comparing investments levels and tax rates across countries is muddled by differences in product and product quality across countries, even within sectors. I am able to show trade-offs between tax rates, front-loadedness of tax payments, risks, progressivity, and investment not only within a single and relatively homogenous commodity, but within the individual financial arrangements between host countries and multinational companies.

I argue that political regimes as well as political time horizons affect both governments' ability to secure their interests in contract negotiations and the trade-offs that they make between various dimensions of value. I find that democracies prioritizing increasing investment levels by reducing tax rates and claims to early revenues; they also assume lower levels of project risk. As time horizons lengthen, democratic leaders further reduce claims to early revenues, though they do begin to increase tax rates, and they use the opportunity to further secure their interests offered by high oil price environments to secure higher investment guarantees from foreign companies. More autocratic countries, by contrast, emphasize collecting more revenues and more early revenues, even if this implies attracting lower levels of investment, and autocratic leaders with insecure tenure emphasize opportunistic early revenue grabs even more so. Democracies, in other words, make policy choices that promote proving more units of oil in the ground, while autocracies make policy choices that promote claiming higher shares of rents from each unit of oil that comes out of the ground.

Although this is consistent with a long literature that argues that democracies have more encompassing interests and therefore fewer incentives to expropriate and more incentives to promote economic growth (Olson 1993), it is at odds with a prominent literature in international political economy linking democratic institutions to higher bargaining power vis-a-vis foreign investors via their better ability to make credible commitments (e.g. Li 2006). Although democracies do behave

less opportunistically than autocracies within the oil sector, they do not necessarily capitalize on improved credibility to set higher tax rates. Instead, democracies enact policies that should encourage even higher levels of investment, by setting low tax rates and by allowing companies to recover sunk costs before taxing oil income. This is especially true among more institutionalized democracies (which should have an even stronger ability to make credible commitments). While this study has focused on oil, the theoretical discussion could be applicable to any dimension of policy-making that requires governments to make trade-offs over time while managing time-inconsistency dilemmas.

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2.9 Appendix

2.9.1 *Types of Fiscal Systems*

To the extent that studies of oil consider differences between countries in methods of revenue collection at all, most focus on whether or not resources are state-owned (e.g. Jones Luong and Weinthal 2010).[53] However, given that the state retains ownership rights under production-sharing arrangements, joint ventures, service agreements, and full nationalization, ownership alone doesn't tell us much about the government's claim to rewards and their exposure to risk. Instead, it is necessary to look deeper at the actual fiscal terms that governments set to govern oil extraction to understand the division of costs and benefits between MNCs and HCs. This is the strategy that I employ in the paper. However, I can use the data collected to elucidate what types of fiscal systems are employed across countries. There are four main ways that governments structure relationships between governments and the companies extracting oil:

- Royalty / Tax Systems: Under royalty / tax systems, companies contract for the right to explore for and produce oil in a particular geographic area. The HC is entitled to a share of oil production, a royalty, and the IOC is entitled to ownership over the remaining share of production. The IOC markets its share of production and pays income taxes on the profits from the sales. Under these arrangements, ownership rights to the oil lie primarily with the IOC, as does the exploration risk and the risk from oil price fluctuations.

[53] Jones Luong and Weinthal (2010) actually make a more nuanced distinction: they look at whether the states owns the majority shares in the petroleum industry (> 50 percent) and whether the state maintains operational and managerial control throughout the project. However, this distinction still masks considerable variation and nuance. For example, they code Brazil as having an ownership regime that qualifies as state (rather than private) ownership with control, their least preferred ownership regime. Although Petrobras, Brazil's state-owned oil company, operates many of Brazil's oil fields, Brazil has also opened up many of its bidding rounds to foreign, private investors as royalty / tax concessions (i.e. without any Petrobras participation), particularly those with more difficult geological conditions (e.g. the 1999 deepwater round). Moreover, Petrobras itself isn't fully state-owned. In 2000, Brazil sold shares of Petrobras, reducing its stake in the company to 32 percent. In 2010, the government held another share offering and purchased additional shares itself, raising its ownership to 54 percent. Although Petrobras has a monopoly on the ability to grant licenses in the oil industry and is state-owned, the extent to which is has participated in projects has varied, as has the extent to which the government actually owned Petrobras itself.

- Production-Sharing Contracts: In a production-sharing contract, the HC collects revenues by assuming a share of the total profit oil. Under these arrangements, the IOC still bears the risk and covers costs. These contracts may additionally collect revenues through royalties and income taxes. Ownership rights to the oil are shared between parties and reflect the share of production accruing to each party. Division of risk depends on how the specific contract allows for costs to be recovered (discussed below).

- Joint Venture Agreements: Joint venture agreements arrange for some degree of risk-sharing between NOCs and IOCs. The degree of risk-sharing varies based on how the ownership is allocated between parties and on the specific terms of the agreement. For example, many joint venture agreements only obligate the NOC to pay their share of exploration costs in case a commercial discovery is made. Joint venture agreements can also utilize royalties, income taxes, and production-sharing to collect revenues.

- Service Agreements: In service agreements, the IOC provides the capital for exploration and development, and, in return, the HC pays the IOC a fee for their services, usually based on a percent of production. The HC retains ownership rights to all of the oil under a service agreement.

- Full Nationalization: In a small number of countries, the oil industry has been fully nationalized. In this case, NOCs bear all of the costs for exploration and development of oil field and the risks of oil exploration. They also retain all of the profits from oil development. In this case, there is obviously no contractual arrangement between IOCs and HCs, but the fiscal system could be viewed as one where the HC assumes all of the risk and all of the rewards from oil development.

Figure 2.7: Prevalence of Fiscal Systems Over Time (1974-2004)

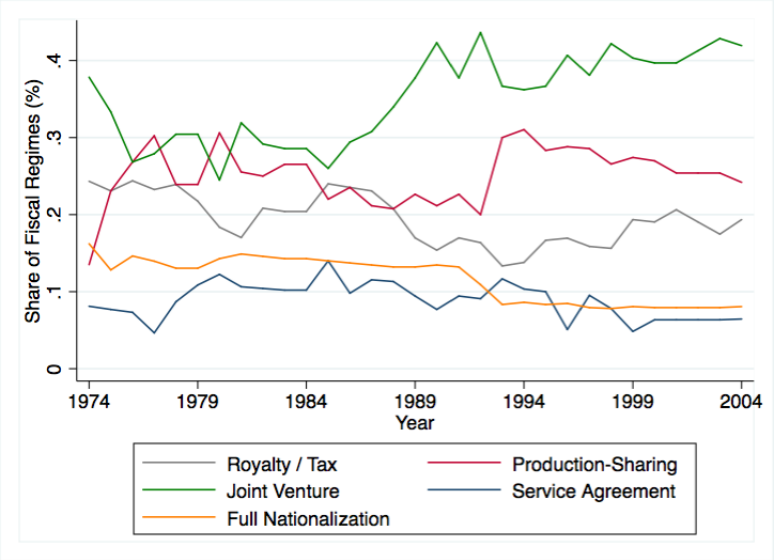


Figure 7 shows the share of countries in the sample that have each of the 5 main types of fiscal regimes in each year between 1974 and 2004. In this figure, a joint venture agreement is defined as any contract where an NOC takes a working interest in oil field development. When countries have multiple forms of fiscal regimes (e.g. both service agreements and joint ventures), both are counted.

Globally, joint ventures are the most prevalent form of fiscal system among developing countries (Figure 7). In 2004, NOCs took a partial ownership stake in 42% of fiscal arrangements. By contrast, service agreements and full nationalizations are the least prevalent form of fiscal regimes. This is in contrast to many discussions of oil contracts and fiscal systems, which cite production-sharing as the most prevalent arrangement (e.g. Aghion and Quesada 2010; Menaldo, forthcoming; Johnston 2007). Which types of fiscal systems are more prevalent in fact depends on how joint venture arrangements are defined. In Figure 1, a fiscal system is defined as a joint venture as any in which governments take a working interest, whether up-front or as a "back-in" option after a commercial discovery is made. Indeed, joint ventures are typically defined using this broad classification (e.g. Johnston 2007).

In Figure 8, I show the prevalence of fiscal systems over time, classifying as joint ventures only those projects in which NOCs take an equity stake from the very beginning of an oil project, i.e. when they are responsible for paying their share of exploration costs even if no commercial discovery is made. Under this classification, production-sharing arrangements are indeed the most

prevalent type of fiscal system. In 2004, 45% of fiscal regimes would be classified as production-sharing arrangements using this definition and only 15% as joint ventures. The differences between Figures 7 and 8 reveal that state ownership in the oil industry can take very different forms across countries, with different implications for the amount of risk born by the state.

Figure 2.8: Prevalence of Fiscal Systems Over Time (1974-2004)

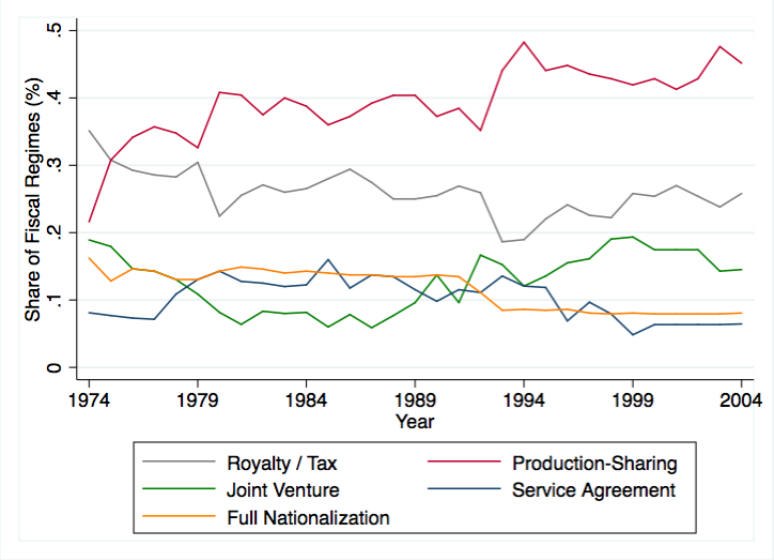


Figure 8 shows the share of countries in the sample that have each of the 5 main types of fiscal regimes in each year between 1974 and 2004. In this figure, a joint venture agreement is defined as any contract where an NOC takes a working interest in oil field development and assumes exploration risk (i.e. the NOC pays its share of exploration costs even if no commercial discovery is made). When countries have multiple forms of fiscal regimes (e.g. both service agreements and joint ventures), both are counted.

Certain fiscal systems are more popular within different regions. Figure 9 graphs the prevalence of different fiscal systems across regions, classifying joint ventures only as those where the government bears exploration risks (as in Figure 8). In all regions except for Latin America and the Caribbean, production-sharing agreements are the most popular arrangement. Latin American countries more frequently employ royalty / tax systems. State ownership is more prevalent in post-Soviet states over the period, reflecting both that these states had fully nationalized oil industries during the Soviet period and are more likely to set joint venture agreements in the post-Soviet period.

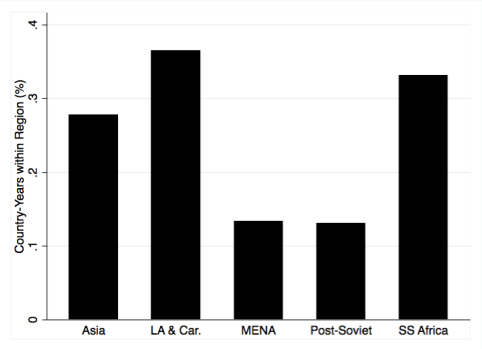
However, all regions employ a variety of different fiscal systems over the period. Moreover,

within fiscal systems, there are a wide range of differences in terms of the risks and rewards allocated between parties. This goes against the idea that there is a single "model contract" or set of fiscal terms that is diffused among HCs.

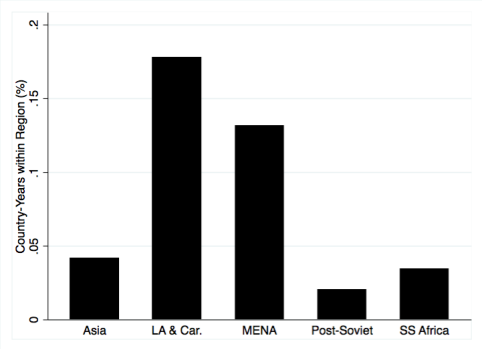
Figure 10 shows the distribution of government take under different fiscal systems. Average government take is similar across systems, and there is wide variation in governments' claim to oil rents within each type of fiscal system. Thus, it is not the case that one type of arrangement is automatically better for governments versus for companies. In theory, contracts in each of these categories could be structured so that they are financially equivalent to each other (Johnston 2007).

Figure 2.9: Prevalence of Fiscal Systems by Region

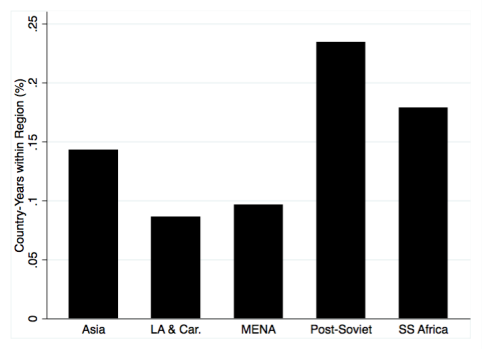
(a) Royalty / Tax Systems



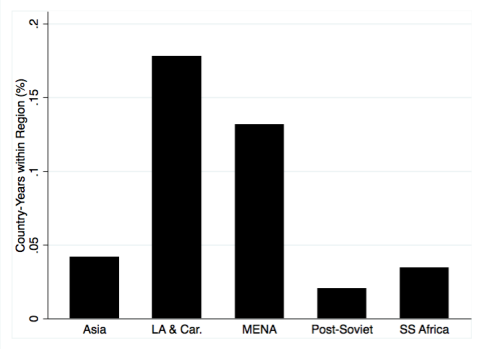
(b) Production-Sharing Agreements



(c) Joint Ventures



(d) Service Agreements



(e) Full Nationalization

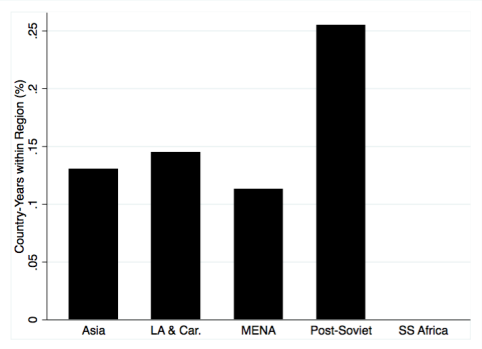
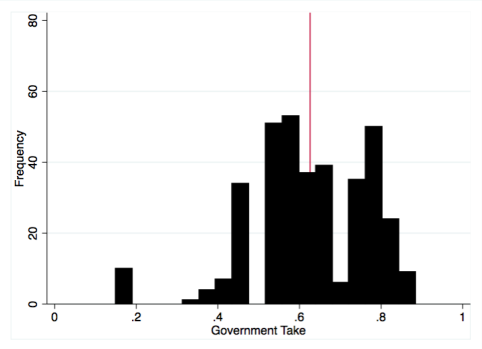
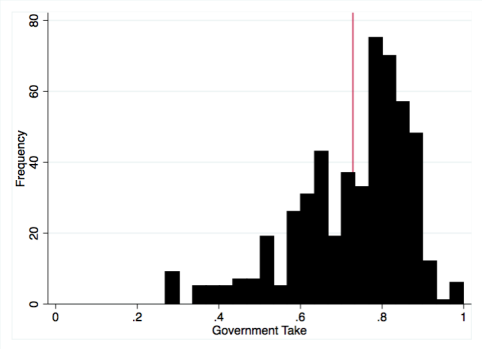


Figure 2.10: Distribution of Government Take within Fiscal System Types

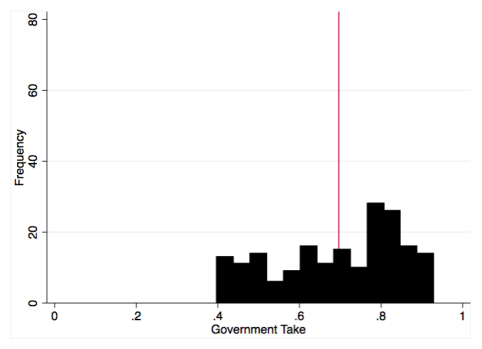
(a) Royalty / Tax Systems



(b) Production-Sharing Agreements



(c) Joint Ventures



(d) Service Agreements

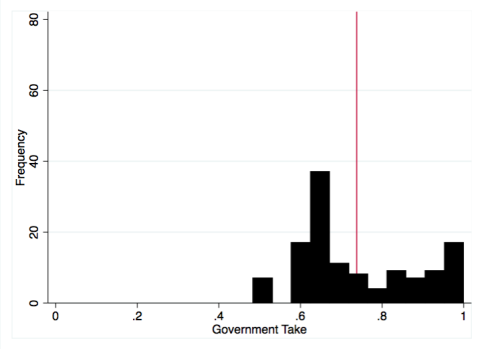


Figure 10 shows the distribution of government take within each type of fiscal regime. Government take is calculated using the "most-likely" scenario for oil prices and costs of extraction for each contract, described in Section X. Red lines indicate the mean government take within fiscal regime type.

Moreover, contracts often do not fit neatly into one of these categories, as governments frequently combine taxation instruments. Take Indonesia as an example. During the early 1990s, Indonesian contracts, often referenced as prototypical production-sharing contracts, in fact combined elements from all four categories. Contracts contained provisions for a royalty, an income tax, and production-sharing. The contracts also allowed for a share of the royalty to go to the company, guaranteeing companies as well as governments early income from oil extraction. This is financially equivalent to a service payment. Finally, Indonesian contracts allowed the Indonesian state-owned oil company, Pertamina, to assume an ownership stake in projects, as in a joint venture agreement. This type of combination of taxation instruments is not unusual. Many countries employ hybrid fiscal regimes.

2.9.2 *Frequency of Changes in Claims to Oil Rents by Law vs. by Contract*

Figure 2.11: Increases in Oil Rents by Law vs. Contract

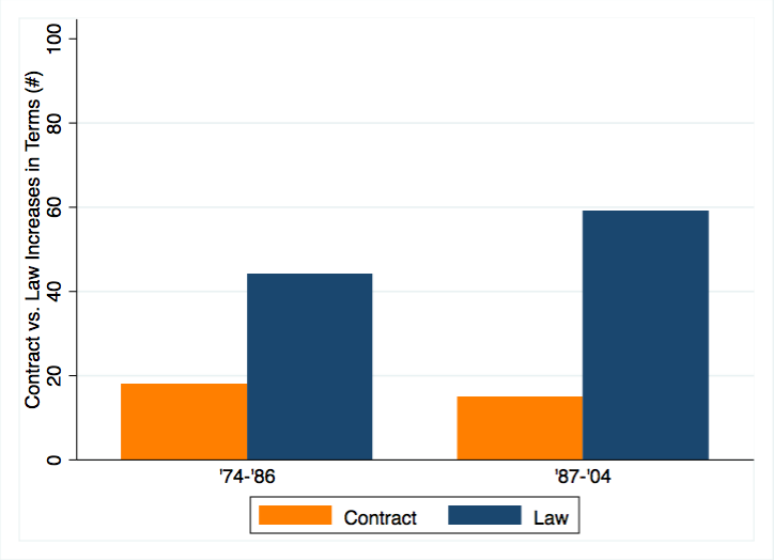
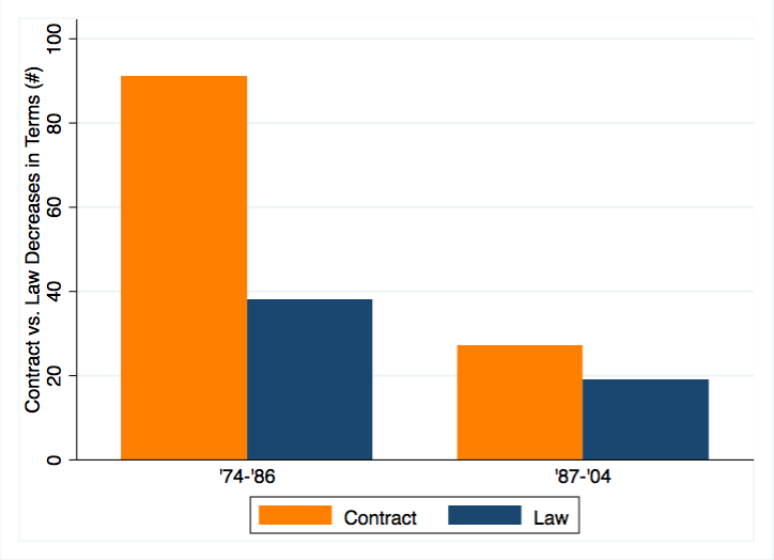


Figure 2.12: Decreases in Oil Rents by Law vs. Contract



2.9.3 *Oil Prices vs. Oil Production*

Figure 2.13: Trinidad and Tobago: Oil Production Increases with Prices

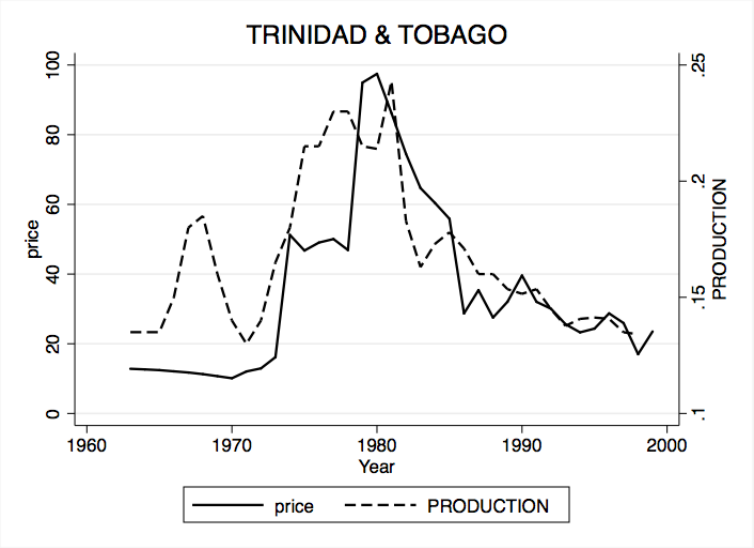
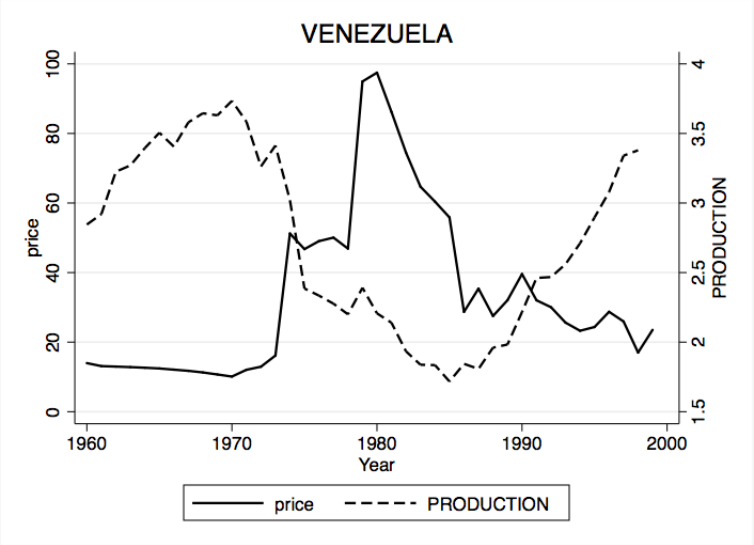


Figure 2.14: Venezuela: Oil Production Decreases with Prices



2.9.4 *Defining the Sample*

Table 2.9: List of Countries that Produce $100 Per Capita in Oil, 1974-2014

Argentina, Azerbaijan, Bahrain, Belize, Bolivia, Brazil, Brunei, Cameroon, Chad, Chile, China, Cote d'Ivoire, Colombia, Croatia, Cuba, Congo (Democratic Republic of), Congo (Republic of), Ecuador, Equatorial Guinea, Egypt, Gabon, Ghana, Hungary, Indonesia, Iran, Iraq, Israel, Kazakhstan, Kuwait, Libya, Lithuania, Malaysia, Mauritania, Mexico, Mongolia, Nigeria, Oman, Peru, Papua New Guinea, Qatar, Romania, Russia, Saudi Arabia, Sudan, Suriname, Syria, Thailand, Trinidad and Tobago, Tunisia, Turkmenistan, United Arab Emirates, Venezuela, Vietnam, Yemen (North and South)

Country-Years in Which Oil Contracts and Hydrocarbons Legislation are Observed

Country	Years
Albania	**1991***, **1993***
Algeria	**1971**, 1975, 1980, **1986**, **1989**, 1993, **1996**, **1997***, 1999
Angola	**1974**, **1978**, 1980, 1981, 1982*, 1984, **1989***, **1995**
Argentina	1968, 1979, 1980, 1986, 1987, **1988**, 1989, **1990, 1991*,** 2001**, 2004**
Azerbaijan	1993, 1994*, 1995*, **1996**, **1997**, 1998, 1999, 2003
Bahrain	1970, 1977, **1979**, 1983*, 1990, 1998
Belize	**1974**, 1978, **1986**, 1988, **1989**, 1993, 1996*, **2000**
Bolivia	**1974**, 1977, 1982*, 1985, **1988**, 1989, **1990**, **1991**, 1992, 1996*, 1997*, 2004
Brazil	1978, 1980, 1997, **1998**, **1999***
Brunei	1968, 1980, 1982, **1990**, 2003
Cameroon	1964, 1978, 1980*, **1990**, 1992, **1998**, **1999**
Chad	1970, 1990
Chile	**1977**, 1988, **1989**, **1993**, 1996, **1997**
China	1970, 1979, 1982, 1983, 1985, 1990, 1993*, 1994, 1995, 1996, 1997*
Colombia	1974, 1977, 1979, **1980***, **1981**, 1982, 1987, **1990**, 1992*, 1994, 1995*, **1998***, 2000*, **2002**, 2004
Congo, Dem. Rep.	1969, 1984, 1987, 1990

Congo, Rep.	1965, 1982, 1985, 1988, 1990, **1993**, **1994***
Cote d'Ivoire	1970, 1980, 1983, 1988, **1990**, **1992***, **1995**
Croatia	**2002**
Cuba	1997
Ecuador	1973, 1974, 1975, 1978, 1982, 1983, **1985***, 1986, **1988**, **1995***, 2002
Egypt	**1974**, **1975**, **1976**, **1977**, 1978, **1979**, **1980**, 1981, 1983*, 1984, 1985*, **1986**, 1987, **1989***, 1993, **1994***, 1995, **1996**, **1997***, 1998, 2002
Equatorial Guinea	1983, 1986, 1989*, **1992***, **1997**, 1998*, 1999, **2000**
Gabon	**1974**, 1982, 1987, 1988*, 1989, **1992***, 1994, **1997***
Ghana	1978, 1985, 1986*, **1988***, 1997*
Hungary	1993, **1995**, 1999
Indonesia	1973, **1975**, **1977**, 1978, **1979**, 1980, 1981, **1982**, **1984**, 1985, 1987, 1988, 1989, 1990, 1991*, 1992, 1993, 1994, **1995***, **1997***, **1998**, 1999, 2003
Iran	**1974**, 1977, 1987, 1997*
Iraq	1972, 1997, **2000**
Israel	1976
Kazakhstan	1992*, 1993*, 1994*, 1995*, 1996, 1997, 2004
Kuwait	**1974**, 1976, **1997**
Libya	**1974**, 1975, 1980, 1990*, **1999***, 2003
Malaysia	1968, 1980, **1987***, **1994***, **1997***
Mauritania	1971, 1988, 1996
Mexico	1938
Mongolia	**1993***
Nigeria	1973, 1975, **1977**, 1978, 1979, **1986***, 1988, 1991, **1994**, 1995, **1998**, **2000**
North Yemen	1974, **1981***, **1985**, 1986, 1987, **1990***
Oman	**1973**, **1974**, **1975**, **1976**, **1978**, 1981, 1989*, 1992*, 1996
Papua New Guinea	**1976**, **1990***

Peru	**1978**, **1979**, **1980**, **1981,** 1982, **1984**, **1985**, **1989***, 1990, **1991***, 1992*, 1993, 1994*, **1995***, 1996, 1997, 2001, **2004**
Qatar	**1974**, **1976**, **1977**, 1985*, 1991, 1994*, 1996*
Romania	pre-1974, **1993**, 1996*
Russia	pre-1974, **1991**, **1992***, **1993**, **1994***, 1995, 1999, 2001
Saudi Arabia	1972, **1974**, 1980, **1997**
South Yemen	1986, 1990
Sudan	1972, 1975, 1980, 1981, **1993***, 1997
Suriname	1965, 1993, 2000, 2003
Syria	1975, **1977**, 1985, **1987**, **1988***, **1989**, 1990, 1992*, 1994, 1995, **1997**
Thailand	1970, **1972**, 1973, **1979**, 1981*, 1982, 1985*, 1989, 1991*, 1994*, 1996*, **1997**
Trinidad and Tobago	1973, 1981, 1985, 1988, **1989***, 1990, **1993***, **1996***, **1998***, 1999*
Tunisia	1972, **1978**, **1980***, 1981, 1984, **1985***, 1987, 1990, 1993, 1994*, 1995, **1999**, 2003
Turkmenistan	**1993***, **1996***, 1999
United Arab Emirates	1972, **1974**, **1975**, **1978**, 1979, **1980***, **1981**, 1982, 1993*
Venezuela	1970, **1975**, 1976, 1993, **1996**, **1997***, 2001
Vietnam	1973, **1978**, 1988, 1991, 1992*, **1993**, **1994***, 1996, 1997, 1998, 2000, **2001**, 2003
Yemen, Rep.	**1991***, 1992, 1993, **1995**, **1996**, **1997**

Bolded years indicate years in which multiple laws or multiple contracts are observed.

Asterisks indicate that the law or contract is observed in both the Johnston database and the Barrows Collection. These sources only overlap in coverage between roughly 1980 and 2000.

2.9.5 *Calculating contract statistics*

Table 2.11: Assumptions used for cash flow analysis of fiscal terms

	Model Assumptions
Field and production assumptions	
Field discovery size (4 scenarios)	50 MMBBL, 100 MMBBL, 500 MMBBL, 1000 MMBBL
Peak production rate	8.5% of field size, in years 7-9
Field decline rate	12.5%
Field life	25 years
Price and cost assumptions (9 scenarios)	
Capital costs as a share of total costs	50%
Total costs as a share of gross revenue	0%, 7%, 15%, 30%, 45%, 60%, 75%, 90%, and 100%

Assumptions are influenced by discussion of average oil field characteristics in Johnston (2003) and Waghorn et al. (2006).

2.9.6 *Autocratic Regime Survival*

Table 2.12: Autocratic Regime Survival, 1960-2006

	Regime Failure
GDP (log)	-0.249 (0.072)**
Growth, t-1	-0.046 (0.011)**
Growth, t-2	-0.002 (0.013)
Islam	0.237 (0.341)
Civil War	0.667 (0.260)*
Military Regime	1.633 (0.302)**
Party Regime	-0.926 (0.274)**
Monarchy	-1.401 (0.580)*
Party-Personal-Military Hybrid	-1.108 (0.768)
Military-Personal Hybrid	0.438 (0.335)
Party-Military Hybrid	-0.263 (0.509)
Party-Personal Hybrid	-0.728 (0.405)+
N	3,006

*Note: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Table report results of a logit model of regime failure. Model includes regional fixed effects for Europe, Middle East / North Africa, Sub-Saharan Africa, East Asia, Central Asia, and Latin America / Caribbean and time splines to control for duration dependence. The excluded regime category is personalistic regimes. See Wright (2008) for further details. Data on autocratic regimes from Geddes et al. (2014).*

Chapter 3

The Struggle for Control Over Aid

3.1 Introduction

By the turn of the twenty-first century, the world's major donors faced a political crisis in aid allocation (Haut 2007). Econometric studies found little evidence that aid achieved developmental objectives (Roodman 2007) and emphasized that donors' strategic interests often distorted the amount of aid allocated to different countries.[1] Additionally, mounting evidence indicated that certain types of aid actually undermined public institutions in aid-receiving countries. In particular, scholars emphasized the negative effects of project aid—aid allocated through relatively small transfers, each with donor-specified objectives, implementing agents, and monitoring and reporting requirements.

Unlike budget support, which finances policies and programs prioritized by aid-receiving governments in national budgets, project aid increases donors' control over aid expenditure (Clist, Isopi and Morrissey 2012; Cordella and Dell'Ariccia 2007; Easterly 2007; Morss 1984). This mode of aid allocation comes at a cost. When donors rather than recipients determine which projects to fund, they short-circuit the "political and managerial processes by which governments think through trade-offs, establish policy priorities, and are held accountable by citizenry" (van de Walle 2001: 209). Profuse project aid also effectively constrains recipient country governments' ability to self-rule: Between 2000 and 2002, for example, the Tanzanian government received more than 1,300 individually specified aid projects requiring more than 3,000 donor meetings and 2,400 reports to donors each quarter. In the face of these crippling bureaucratic requirements, Tanzania declared a four-month period during which it would not write donor reports or accept donor visits so that it would have time to govern (Birdsall 2004).[2]

Additionally, aid reformers lament that Western economists have often imposed misguided policy prescriptions on developing countries that obstruct critical opportunities for local actors to innovate the most appropriate, contextually-specific paths to development (Easterly 2006). Aid would be more effective, reformers argue, if donors delegated greater decision-making power to

[1] For overviews of this large literature, see Alesina and Dollar 2000: 34-35 or Neumayer 2003: 21-9.

[2] Tanzania is not anomalous. For discussion of other cases, see Frot and Santiso 2010: 27-29, and Acharya, Fuzzo De Lima, and Moore, 2006: 2.

local actors.

In response to this criticism, the World Bank and all 26 member states of the Development Assistance Committee (DAC) of the Organization for Economic Cooperation and In response to this criticism, the World Bank and all 26 member states of the Development Assistance Committee (DAC) of the Organization for Economic Cooperation and Development (OECD) introduced a "new aid paradigm" (Dijkstra 2013). Above all, this new paradigm would put recipient country governments "back in the driver's seat" (Wolfensohn 1999) and promote "aid ownership," i.e., the ability of recipient country governments to influence and be held accountable for aid expenditures within their borders (OECD 2005).[3] Following the World Bank's implementation of Poverty Reduction Strategy Projects (PRSPs) in 1999, DAC donor country governments committed to improve aid effectiveness in part by allocating more lump sum, sector-wide support or budget aid (OECD 2005). Donors noted, however, that in extremely weak states or in states ruled by undemocratic governments, it might be necessary to withhold aid ownership from country governments to ensure that aid is actually spent on "broad-based" development (Oxford Policy Management/IDL 2008; World Bank 1998).

This paper evaluates the conditions under which donors have in fact been willing to allocate greater aid ownership to recipient countries. Despite its relevance for aid policy, scholars have only recently begun to investigate questions concerning why donors vary aid composition (i.e. aid delivery tactics or modalities). As we explain below, this literature largely assumes that donors are "outcome-oriented" and that their "development strategy seeks to maximize the impact of their aid on recipient development" (Dietrich 2013, 698). Yet, this conflicts with the OECD's (2011) "sobering" self-assessment that DAC donors have failed to increase aid ownership for aid effectiveness. What explains this divergence?

We argue that donors face conflicting incentives when they select aid delivery tactics. In addition to donors' interests in promoting conventional development goals – such as economic growth

[3]Accordingly, the OECD favors the term "partner countries" over "aid recipients." Below, we argue that donors strategically vary the degree to which they treat country governments as partners. We therefore use "recipient," "aid-receiving country," and "country government" interchangeably.

and more effective, democratic governance – donors also face incentives to vary aid composition in pursuit of foreign policy and security objectives. As these logics are not mutually exclusive, we evaluate the relative weight of each. In accounting for donors' diverse incentives, we provide a comprehensive analysis of DAC donors' allocation of aid ownership.

We also contribute by demonstrating that measures employed in recent studies are not reliable indicators of aid ownership without also accounting for the extent to which aid is fragmented across projects. Consistent with policymakers' characterization of highly fragmented aid as low ownership aid, we construct a Hirschman-Herfindahl index of aid concentration that evaluates the extent to which donors allocate aid in lump sum versus fragmented transfers. Enlisting this measure alongside of two alternative measures, we find robust evidence that donors' foreign policy and security interests are stronger and more consistent predictors of aid composition than recipients' capacity or commitment to liberal development policy. Further, we find no evidence that donors have become increasingly responsive to developmental concerns over time.

These findings have implications for aid policy. As Easterly and Pfutze (2008) have underscored, successful aid reform depends on our understanding of donors' (in)ability to meet their public commitments in the past. It is to this end that we examine donors' ability to adhere to their own commitments and seek to identify the obstacles that they face. We do not, however, assume that adherence to donor-specified best practices would positively affect development. While there is evidence that highly fragmented aid harms developing countries' institutions, we lack evidence that "high ownership" aid is effective. Below, we discuss how bilateral aid is allocated and introduce recent research on aid composition. In Sections 3-5 we develop and test the argument that donors face conflicting incentives when allocating aid ownership. We conclude in Section 6 with discussion of the policy implications of our findings.

3.2 Aid, selectivity, and composition

When allocating aid, donor country governments make decisions over aid targeting and aid composition. First, donors identify the countries to which they will allocate aid and in what amount. Second, donors specify aid composition (i.e., delivery tactics) by indicating how donors will commit, disburse and evaluate aid to each recipient. On the first question, U.S. congressional leaders, for example, routinely earmark large sums of money for politically salient recipients, while simultaneously allocating funds for development agencies, such as USAID, which distributes its funds according to criteria developed in part by the State Department. Although additional funds are targeted to multilateral organizations, such as the World Bank, bilateral government-to-government transfers accounted for roughly 75% all development aid between 2000-2010 (OECD 2011).

What drives donors' aid targeting decisions? Although OECD donors have often claimed to condition aid amount on recipient country policy, numerous studies have found that they are generally unable or unwilling to do so. This may be caused by information and agency problems inherent in foreign aid (Martens et al. 2002; Easterly 2006); the moral imperative and "warm glow" that many donor country citizens feel when allocating aid, which makes aid difficult to withdraw (Andreoni 1990); or the strength of donors' strategic interests in recipient countries (Dunning 2004; Stone 2006; Bueno de Mesquita and Smith 2009; Fleck and Kilby 2010). While there is evidence that some donors target aid towards relatively poor countries (Clist 2011), there is only weak evidence that donors allocate more aid to recipient countries with better policy environments for development.

Yet, even if donors do not effectively condition aid amount on recipient country policy, they may still exercise selectivity by varying the composition of their aid (Radelet 2004, Birdsall, Kharas and Perakis 2010). As Clist and coauthors note, "the policy lever for dealing with low levels of governance is the type of aid delivered, specifically the amount of control a recipient is granted" (2012:268; Radelet 2004:12, Birdsall and Kharas 2010:4). By varying aid composition, donors can effectively constrain the ability of country governments to divert aid expenditure from donors' objectives.

How do donors vary aid composition? Historically, scholars have identified budget aid and project aid as two "extremes of control" (Clist, Isopi, and Morissey 2012:269; Cordella and Dell'Ariccia 2007). At one extreme, donors allocate large, lump sum transfers of general budget support, which enable country governments to allocate and administer aid. At the other extreme, donors spread aid across a large number of small projects. Project aid increases donor control over expenditure because donors specify the intended objectives, beneficiaries, and disbursement, monitoring and reporting requirements associated with each project. Empirical evidence confirms that more precisely targeted projects experience less diversion of funds from donors' preferred objectives (Winters 2014).

Given the prominent view that greater recipient country ownership increases aid effectiveness, a surprisingly low share of foreign aid is allocated as budget support. The OECD (2011) estimates that most donor countries direct less than 10% of foreign aid to general or sectoral budget support. Instead, since the end of the Cold War, donors have dramatically increased the percentage of bilateral aid allocated as project aid. Simultaneously, the average size of DAC donors' projects decreased: Over the last 20 years, the average value of an OECD bilateral foreign aid project has fallen from roughly $10 million in 1991 to less than $1 million in 2011.[4] Moreover, even when allocating budget aid, Dijkstra (2013) notes that DAC donors increasingly allocate small budget aid transfers alongside high volumes of project aid. After endorsing a PRSP in 2001, for example, Mozambique received 85 budget aid transfers between 2001 and 2010, but this accounted for only 18% of the country's total bilateral aid (Batley 2005).[5]

Why have donors increasingly allocated fragmented project aid? Explanations may include increasing competition among donors as more countries and agencies have entered the aid "market"; the related tendency for traditional donors to allocate aid to larger numbers of countries (Frot and Santiso 2009); the proliferation of specialized bureaucratic units within donor country governments, each with incentives to allocate and monitor projects (Cooley and Ron 2002; Kilby

[4] Author's calculations.

[5] This may explain why Clist, Isopi and Morissey (2012) find that the factors that predict the allocation of (any) budget aid by multilateral donors do not also predict the share of budget aid allocated.

2011); and a shift towards aid in social sectors, which may be prone to fragmentation (Frot and Santiso 2010). It is likely that all of these factors contribute to relatively high levels of project fragmentation.

If donors were adhering to their own commitments to promote aid ownership, we would expect a decrease – not increase – in project fragmentation over time. Yet, recent studies argue that, even when allocating project aid, donors may still exercise selectivity in favor of aid effectiveness. Dietrich (2013), for instance, argues that donors have pursued greater aid effectiveness by exercising selectivity over project implementing agents. When donors perceive recipient country governments as capable and well-governed, Dietrich argues, donors are more likely to send aid through recipients' public sectors. By contrast, donors tend to "bypass" corrupt or weak governments by allocating project aid through NGOs or private contractors. Similarly, Knack (2013) suggest that donors are unlikely to utilize recipient countries' financial systems to administer aid if donors perceive recipients' systems as low capacity.

While the links between good governance, allocation of budget aid, and allocation of aid through public sectors offer suggestive evidence that donors are ceding more control over aid to countries with better governance, these studies overlook other motivations for donors to vary recipients' control over aid expenditures. A vast literature on aid conditionality finds that donor governments' strategic interests often distort aid targeting. In the following section, we argue that donors also face incentives to vary their aid delivery tactics in order to achieve diverse aid objectives, including but not limited to development.

Additionally, it is important to recognize that while donors' willingness to engage recipient country governments in project implementation and aid administration is important, it does not reliably capture donors' willingness to cede ownership over aid to recipient governments. In Tanzania, for example, donors have channeled high volumes of aid through the Tanzanian government but divided that aid across numerous donor-specified projects, each with costly monitoring and reporting requirements. In 2006, for instance, at least 73% of bilateral aid to Tanzania was channeled through government systems but was fragmented across 663 separate donor-specified aid

projects with a median size of less than $120,000. Tanzania is thus considered a classic case of low ownership because of the fragmented way in which donors engage country systems (Birdsall 2004).

British aid to South Africa in 2008 provides a contrasting example. Although the UK did not allocate budget support, many of its projects were channeled through the public sector in relatively large transfers.[6] The most prominent project supported a Rapid Response Health Fund to empower the South African Minister of Health to be able to rapidly address problems that may arise in the implementation of HIV/AIDS programming. This type of aid, by explicitly empowering local actors, reflects significantly more government influence over expenditure than the profuse project aid allocated to Tanzania. Yet, considering budget support alone, British aid to South Africa appears identical to aid to states where local actors are bypassed entirely (both receiving zero budget support). By contrast, considering whether donors engage or bypass recipients' public sectors flattens the distinction between high and low ownership aid – a central theme in aid policy debates.

In what follows, we offer a more comprehensive account of donors' motivations to vary aid ownership. To test our argument, we examine the extent to which donors spread aid across many, small projects, a continuous indicator of the extent to which donors relinquish control over aid expenditures to aid-receiving countries.

3.3 Incentives for donors to allocate aid ownership

3.3.1 *Aid ownership for development*

DAC donors have taken a strong, public stand on the relationship between aid ownership and aid effectiveness. Ensuring ownership over aid for aid-receiving countries "increases aid effectiveness by strengthening the partner country's sustainable capacity to develop, implement and account for

[6]Clear data on implementing agency is missing for 87% of the total aid for this donor-recipient dyad, so it is difficult to specify exactly how much aid was channeled through the public sector.

its policies to its citizens and parliament" (OECD 2005: 4). Yet, donors simultaneously acknowledge that many developing countries lack the institutional capacity to ensure that aid reaches its intended beneficiaries. For instance, in states with low-functioning and corrupt bureaucracies like Chad and Uganda studies have found that 99 percent of central government expenditures failed to reach school and health clinics (Gauthier and Wane 2009; Reinikka and Svensson 2004). Accordingly, to prevent aid capture, it may be necessary for donors to retain control over aid in countries with weak institutional environments.[7]

Additionally, in their most robust statement of developmental commitments—the Paris Declaration (OECD 2005)—DAC donors acknowledge that for aid reforms to work, aid-receiving countries must have broad-based national development strategies in place. Undemocratic governments and governments that invest more heavily in patronage and the military than health and education are likely to spend budget support on similar priorities. Donors motivated by aid effectiveness may therefore allocate fragmented project aid in order to prevent aid capture by governments whose demonstrated commitment to liberal development policy is low.

DAC donors' own "best practices" therefore imply that when donors are motivated primarily by aid effectiveness, they should allocate high ownership aid on average. Donors may deviate from this high ownership mean by restricting opportunities for aid ownership only when governments suffer from extremely low capacity or lack a demonstrated commitment to liberal development priorities (hereafter, commitment to development).

To evaluate government capacity and quality, we follow Langbein and Knack (2010) in using the mean of the six dimensions of the World Governance Indicators (Kaufmann, Kraay and Mazruti 2010). To capture countries' commitment to development, we consider three measures. First, we assess democratic institutions using the Freedom House Index. To ease interpretation, we reverse the scale of the Freedom House Index so that lower values indicate less democratic institutions. Second, we include a dummy variable that indicates whether a country has endorsed a Poverty

[7]This behavior can be self-defeating over the long run, as allocating highly fragmented aid can further weaken institutions, Yet, individual donors face incentives to avoid allocating high ownership aid in the near term when other donors will reap related gains without paying the costs of higher risk of aid capture in (Knack 2013).

Strategy Reduction Paper (PRSP). Introduced in 1999 by the World Bank and IMF, PRSPs outline "a country's macroeconomic, structural, and social policies and programs to promote growth and reduce poverty, as well as associated external financing needs." Typically, governments work with donors to develop their PRSPs, which also make them eligible for debt relief and concessional financing under the World Bank and IMF's Heavily-Indebted Poor Countries Initiative. We therefore view the publication of a PRSP as indicating a government's committed to development spending that is in line with the priorities of major liberal donors. Third, given the OECD's repeated emphasis on education as a cornerstone for successful recipient-led development, we consider government spending on education (per capita) as a share of GDP.

3.3.2 *Aid ownership for influence*

In contrast to the view that donors are primarily motivated by concerns about promoting development, numerous studies of aid targeting emphasize that donors use foreign aid as a policy tool to pursue foreign policy or military objectives (e.g., Bueno de Mesquita and Smith 2009). Granting – or withholding – control over aid expenditures is key to achieving foreign policy or military objectives through foreign aid. Specifically, when strategic interests are at stake, donors face incentives to allocate ownership only when country governments demonstrate commitment to shared strategic interests. In these cases, donors stand to benefit by allocating high ownership aid so that local actors can effectively direct aid dollars towards shared security objectives. Between 2000 and 2004, for instance, the world's largest bilateral donor, the U.S., allocated 90% of its bilateral development aid to Israel – a critical military ally – in lump sum transfers of budget aid.[8]

By contrast, in the absence of shared interests, donors would reasonably expect aid-receiving countries to divert aid from donors' objectives if given high ownership aid. Accordingly, the U.S., has allocated fragmented project aid to non-state actors in countries where governments oppose

[8] After 2004, aid allocated to Israel (and other countries with relatively high GDPs) could no longer be counted as "Official Development Assistance."

U.S. strategic interests, such as Russia and Bolivia, and in countries where the U.S. had major foreign policy interests in political transitions, i.e., Egypt after Mubarak's fall. The governments of these countries expelled USAID after accusing the U.S. government of undermining their ability to self-govern. In these cases, low ownership aid was used strategically to achieve foreign policy and security objectives.

We therefore expect that donors motivated primarily by foreign policy and security objectives will allocate relatively low-ownership aid on average, and will deviate from this mean when they seek to induce or reward governments for embracing donors' strategic priorities. To evaluate this argument, we consider the influence of United Nations General Assembly (UNGA) voting alignment and donors' official arms transfers on the allocation of aid ownership during the implementation of the "new aid paradigm" (2000-2010). Voting in the UNGA gives donors a unique opportunity to observe governments' relative policy positions in the same setting (e.g., Dreher, Nunnenkamp and Thiele 2008; Dreher and Sturm 2012; Kuziemko and Werker 2006). According to the U.S. Department of State (1985), for example, UNGA voting enabled them to "make judgments about whose values and views are harmonious with our own, whose policies are consistently opposed to ours, and whose practices fall in between" (as quoted in Dreher and Jensen 2013). We expect that donors will allocate higher levels of aid ownership to UNGA voting allies. To measure UNGA voting alignment, we favor a simple proportion of shared votes between each donor and recipient pair (Voeten, Strezhnev and Bailey 2013).

Additionally, when donors' security interests are at stake and they view recipient country governments as reliable partners, we expect donors to allocate higher ownership aid. When donors seek to bolster the military and political strength of an aid-receiving government, they may actually support the diversion of aid dollars away from development objectives towards other political or military tasks, such as supplying local militias with resources or buying off internal opposition. In these cases, donors prefer to allocate money in relatively large transfers with fewer specified development objectives and reporting requirements in order to maximize the discretion of recipient governments over expenditure. To measure donors' interest in the military cooperation of recipient

countries, we construct a binary indicator equal to one if a donor transferred arms to a recipient's government in a given year (SIPRI 2012).

In using UNGA voting alignment and bilateral arms transfers as indicators of donor-recipient alignment on foreign policy and security goals, we note the difference between strategic alignment as opposed to salience of recipient countries. Though several studies use United Nations Security Council (UNSC) membership as an indicator of donor "security interests", UNSC membership alone does not signal policy alignment. For example, Russia and China are permanent members of the UNSC whose foreign policy and security interests deviate substantially from OECD donors. While the increased authority of UNSC members may induce donors to target them with aid, we expect donors grant ownership only when they perceive interest alignment.

3.4 Measuring aid concentration

In line with the view that donors' tendency to spread aid across many separate projects reduces aid ownership for aid-receiving countries[9], we measure aid ownership by the extent to which aid is "concentrated" in a few projects versus "fragmented" across many projects using a Hirschman-Herfindahl Index:

$$Aid\,Concentration = \sum_{i=1}^{n} \left(\frac{P_i}{A}\right)^2$$

where P_i is the amount of aid allocated for project i, and A is the total aid allocated by a donor to a recipient in a given time period. Project-level data on bilateral aid commitments comes from AidData 2.0 (Tierney et al. 2011). We include all 22 DAC donor countries in our sample of donors and all countries that received Official Development Assistance from these donors, 2000-2010.[10]

[9]Theoretically, fungibility could mean that there is no difference between lump-sum and project aid other than transaction costs. If aid funded projects that recipient governments would otherwise have implemented, this would free up government resources. Yet, this is only true when donor and recipient governments share preferences; we argue, that donors are more likely to allocate project aid when their preferences diverge from recipients.

[10]DAC donors listed in the Appendix.

Aid concentration values range between 0 and 100. A value of 100 indicates a perfectly concentrated aid commitment, or, aid allocated in a single lump-sum payment. To ensure that the aid concentration scores are meaningful, we take four precautions. First, we exclude all projects less than $10,000, as they may reflect donor expenses related to aid management, which some donors are more likely to report as aid projects than others (Togo and Wada 2007). Second, countries receiving a small total amount of aid may receive aid concentration scores with little substantive meaning. We therefore exclude all dyads receiving less than $1 million in bilateral aid in a given year.[11]

Third, in calculating aid concentration, we ensure that the types of aid flows included in the analysis truly represent positive bilateral aid flows between donors and recipients that actually take place in the recipient country. To do this, we exclude Official Development Assistance classified as debt relief, donor administrative costs, and aid to refugees living in donor countries. The distribution of aid concentration is reported in the Appendix.

Finally, we address the concern that aid concentration and aid amount correlate. Figure 1 plots aid concentration values against the logged value of total annual bilateral aid allocated to aid recipients. The correlation between aid concentration and aid amount is close to zero ($r = -0.20$). This mitigates the potential concern that the relationship between the explanatory variables and Aid Concentration is mediated through aid amount.

[11] In the Appendix, we show that the results are not sensitive to these thresholds.

Figure 3.1: Aid Concentration vs. Aid Amount

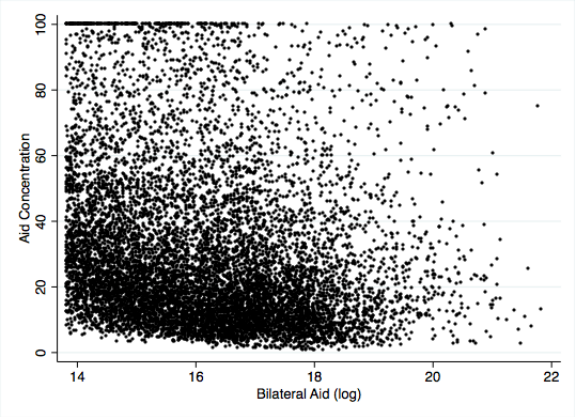


Figure 1 plots aid concentration scores against the logged value of total annual bilateral aid allocated to aid recipients for all recipients in our sample.

3.5 Results

3.5.1 *Overall rates of aid concentration*

We begin by examining overall rates of aid concentration. On average, do donors allocate high ownership, concentrated aid or low ownership, fragmented aid? Rates of aid concentration are remarkably low: In 2010, the median aid concentration value was only 19.4. To put this in context, Swedish aid to Uganda in 2010 receives an aid concentration score of 19.6, which represents around $58 million in positive aid flows spread across 39 separate projects of an average size of around $1.5 million. It is staggering to consider that this represents aid flows from a single donor: In 2010 alone, Uganda received 1,148 separate aid projects from bilateral donors. This hardly represents the type of high ownership aid advocated by the Paris Declaration. Figure 2 presents the distribution of aid concentration over time. If anything, the figure shows a slight downward trend in aid concentration over time. Thus, it is not the case that donors are on average allocating more aid concentration in response to their own commitments to aid ownership.

Figure 3.2: Aid Concentration Over Time

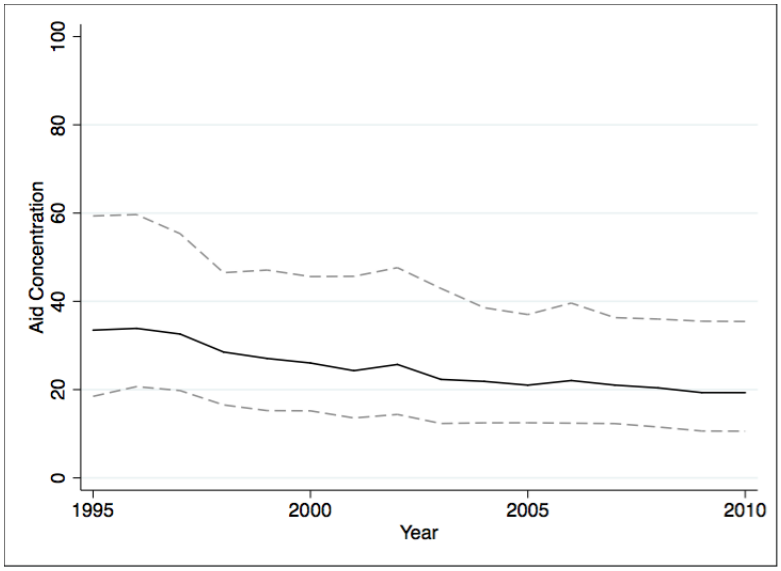


Figure 2 shows the median aid concentration value for all DAC donors by year between 1995 and 2010. The dotted lines represent the 25th and 75th percentile aid concentration values.

Yet, overall low rates of aid ownership among aid-receiving countries do not necessarily indicate that donors are failing to adhere to their own principles about aid effectiveness. High ownership may only enhance aid effectiveness when aid-receiving countries have strong governance and high commitment to pro-development spending. Low average levels of ownership may simply reflect the failure of most aid-receiving countries to meet these conditions. Below, we assess this possibility empirically.

3.5.2 *Analysis*

The unit of analysis is the donor-recipient dyad year over the period between 2000 and 2010. We include only donor-recipient-years with positive bilateral aid flows. Therefore, we should interpret the analysis as the factors that influence aid ownership conditional upon receiving aid. We log the dependent variable so that it is normally distributed.[12] This transformation also ensures that the dependent variable is not bounded, allowing for straightforward estimation through OLS.

[12]The distribution of the logged dependent variable is reported in the Appendix.

We present two types of models. First, we show fixed effects models. Fixed effects models have the advantage of controlling for potential confounds that do not vary over time. We also report models that instead include year and dyad fixed effects. These models have the advantage of holding constant all unique features of the relationship between donors and recipients (e.g. trading relationship, colonial relationship, cultural exchanges, historical features of the foreign aid relationship).

While fixed effects models are typically the gold standard of time series cross-sectional analysis, their distinct disadvantage is that they only enable us to estimate the effects of changes in covariates within aid-receiving countries, in other words, the "within" country effects. These effects are interesting for the political and security variables considered here, which do vary over time. However, capacity and commitment to development tend to be slow moving, and fixed effects do not allow us to estimate the effects of differences between countries in these slow-moving covariates.

In the Appendix, we summarize the variables included in the analysis, including the proportion of the variance that occurs between donor-recipient dyads. Around half of the variance in aid concentration occurs between donor-recipient dyads. Thus, fixed effects models that only consider variation within dyads miss approximately half of the variation in aid allocation behavior. Further, many of our key explanatory variables are slow-moving, varying primarily between- rather than within-dyads.

This suggests that random effects models are appropriate. However, typical random effects models suffer from potential correlation between covariates and residuals. Bell and Jones (2015) argue that this problem most often arises because, in reality, each covariate is composed of two separate parts: a "within" and a "between" effect. In other words, typical random effects models assume that a one-unit change in a covariate within a single country has the same effect as a one-unit difference in a covariate between countries. If these two processes differ, then any unaccounted for variance is pushed into the error term, causing omitted variable bias. The authors argue that by separately including the "within" and "between" effects within the random effects models, most

omitted variable bias and endogeneity problems with time series cross-sectional data are resolved. Most importantly, this specification enables us to estimate the effects of slow-moving covariates through the "between" effects.

We therefore additionally estimate the following within-between random effects model for donor i, recipient j, and time period t, we estimate the model:

$$ln(Aid\,Concentration)_{ijt} = \alpha + \beta_1(X_{ijt-1} - \bar{X_{ij}}) + \beta_2\bar{X_{ij}} + \beta_3(Z_{jt-1} - \bar{Z_j})$$

$$+\beta_4\bar{Z_j} + \nu_t + \eta_t + (\varepsilon_{ijt} + u_{ij})$$

where $Aid\,Concentration_{ijt}$ is the concentration of donor i's bilateral aid to recipient j in year t; $\bar{X_{ij}}$ are the means of each covariate x_{ij}, as such the time-invariant component of these variables ("between" effects); $X_{ijt-1} - \bar{X_{ij}}$ is the difference between the lagged covariate and the mean value for dyad ij ("within" effects); $\bar{Z_j}$are the means of each covariate z_j, as such the time-invariant component of these variables ("between" effects); $Z_{jt-1} - \bar{Z_j}$ is the difference between the lagged covariate and the mean value for recipient country j ("within" effects); and ν_t and η_i are year and donor fixed effects respectively.

Table 1 presents the results from both fixed effects and random effects specifications. The dependent variable in all models is log-transformed. One can therefore interpret a regression coefficient as a $100 * (e^{\beta} - 1)$ percent change in aid concentration for each one-unit increase in the explanatory variable. The first column reports OLS results estimating the logged concentration of aid delivered by donors to recipients, utilizing donor, recipient, and year fixed effects (Model 1). The second column utilizes dyad and year fixed effects (Model 2). Column 3 reports a standard random effects model, with random effects by aid-receiving country and donor and year fixed effects (Model 3).

Table 2 presents the results from the within-between random effects specification. Column 4 reports a within-between random effects specification, with random effects by aid-receiving coun-

try and donor and year fixed effects (Model 4). One issue that arises in the interpretation of the results for arms transfers is that not all donors in the sample report allocating arms transfers during the period. This can make interpretation of this variable difficult, as zeroes could occur either because the donor never makes arms transfers or because a donor selected not to transfer arms to a particular country. Thus, in Model 5 we include only the donors that report at least one arms transfer in the sample during the period.[13] Model 6 excludes GDP per capita as a control in order to observe the effects of recipient state capacity more easily.

All models also include controls. We control for civil conflict, as donors face obstacles to allocating high ownership aid to countries immersed in conflict. We also control for colonial ties between donors and recipients. Donors plausibly have greater information about former colonies, which could shape their willingness to grant aid ownership. Additionally, we control for total amount of aid allocated bilaterally by all DAC donors to aid-receiving countries and the number of donors operating within a country. Although donors may wish to coordinate and harmonize aid behavior, in practice each individual aid agency faces bureaucratic incentives to implement its own projects in order to maintain funding and relevance (Cooley and Ron 2002).

We also control for trade intensity between donors and recipients, the logged sum of imports and exports (Barbieri et al. 2008). Donors with greater economic interests in recipient countries may be more likely to allocate high ownership aid to promote long-term economic development. Logged population and logged GDP per capita also serve as controls. We also control for UN Security Council membership, in case donors allocate greater aid ownership in order to obtain support on the Security Council. Finally, to measure the importance of the recipient to the donor we control for each recipient's rank in each donor's aid portfolio in a given year, with higher rank indicating that the recipient received more aid.[14]

[13] Listed in the Appendix.

[14] We favor this measure over the direct inclusion of the amount of bilateral aid. We view aid allocation as a three-step process in which donors decide whether to give aid, how much aid to give, and, finally, the mode of aid allocation (as in Clist, Isopi, and Morissey 2012). If, instead, one viewed the decisions of how much aid and the mode of aid as simultaneous decisions, then including the amount of aid would induce bias. We thus err on the side of not inducing bias in the estimates and exclude aid amount from the models. The Appendix demonstrate that including aid amount does not alter findings.

Table 3.1: Explaining Aid Ownership, Development vs. Influence, 2000-2010

	Model 1 FE	Model 2 FE	Model 3 RE
L.WGI	0.085 (0.053)	0.076 (0.063)	0.070 (0.042)+
L.PRSP	-0.000 (0.030)	-0.031 (0.032)	0.013 (0.027)
L.Freedom House	-0.006 (0.012)	0.002 (0.012)	-0.011 (0.010)
L.Educ. Spending (% GDP)	-0.018 (0.005)	-0.019 (0.005)**	-0.011 (0.003)**
L.UNGA	0.515 (0.160)**	0.443 (0.132)**	0.440 (0.140)**
Arms Transfer	0.090 (0.052)+	0.111 (0.046)*	0.100 (0.052)*
UNSC	0.020 (0.037)	0.021 (0.039)	0.017 (0.037)
Former Colony	-0.406 (0.071)**		-0.410 (0.070)**
Civil Conflict	0.008 (0.038)	-0.015 (0.039)	0.007 (0.034)
Total DAC Aid (log)	0.088 (0.020)**	0.030 (0.020)	0.063 (0.017)**
Recipient rank in donor portfolio	-0.004 (0.001)**	0.001 (0.001)	-0.004 (0.001)**
L.Population (log)	0.243 (0.316)	0.133 (0.360)	-0.042 (0.023)**
L.GDP per capita (log)	0.196 (0.114)+	0.217 (0.121)+	0.111 (0.031)**
L.Number of donors	0.001 (0.005)	-0.004 (0.006)	-0.012 (0.005)**
L.Trade Intensity	-0.059 (0.013)**	0.011 (0.015)	-0.056 (0.012)**
Donor Fixed Effects	Y	N	Y
Recipient Fixed Effects	Y	N	N
Dyad Fixed Effects	N	Y	N
Year Fixed Effects	Y	Y	Y
Observations	10,240	10,240	10,240

*Notes: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Standard errors are in parentheses and are clustered by aid-receiving country. For all models, logged aid concentration score is the dependent variable.*

Table 3.2: Explaining Aid Ownership, Development vs. Influence, Within-Between Random Effects Models, 2000-2010

		Model 4 Full Sample	Model 5 w/ Arms	Model 6 w/out GDP
Between Effects				
	WGI	-0.002 (0.070)	0.054 (0.066)	0.132 (0.051)*
	PRSP	0.234 (0.096)*	0.172 (0.094)+	0.124 (0.085)
	Freedom House	-0.042 (0.018)*	-0.039 (0.018)*	-0.047 (0.018)*
	Educ. Spending (% GDP)	-0.007 (0.013)	0.001 (0.014)	-0.010 (0.011)
	UNGA	0.720 (0.299)*	0.711 (0.309)*	0.755 (0.294)*
	Arms Transfer	0.134 (0.094)	0.235 (0.097)*	0.150 (0.094)
Within Effects				
	L.WGI-mean	0.100 (0.063)	0.131 (0.068)+	0.121 (0.067)+
	L.PRSP-mean	-0.025 (0.033)	-0.021 (0.038)	-0.024 (0.033)
	L.Freedom House-mean	0.004 (0.012)	-0.001 (0.013)	0.004 (0.012)
	Educ. Spending (% GDP)-mean	-0.014 (0.006)*	-0.015 (0.006)*	-0.011 (0.005)*
	L.UNGA-mean	0.449 (0.133)**	0.622 (0.147)**	0.458 (0.132)**
	Arms Transfer-mean	0.109 (0.046)*	0.109 (0.046)*	0.109 (0.046)*
	Donor Fixed Effects	Y	Y	Y
	Recipient Fixed Effects	Y	Y	Y
	Year Fixed Effects	Y	Y	Y
	Observations	10,240	8,372	10,240

*Notes: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Standard errors are in parentheses and are clustered by aid-receiving country. For all models, logged aid concentration score is the dependent variable. All models include donor and year fixed effects and random effects by recipient country. Model 5 includes only those donors that record at least 1 arms transfer, 2000-2010. Within and between effects are included, but not reported, for all control variables listed in Table 1 unless otherwise noted.*

3.5.3 *Aid ownership for development?*

Overall, we find limited evidence that aid ownership is shaped by a country's capacity and commitment to development. Capacity, as measured by the WGI governance index, is consistently associated with greater levels of aid ownership, but is not always significant. Because the WGI index is highly correlated with (logged) GDP per capita in the sample (r = 0.60), it is often insignificant when both are included in the same model. In general, per capita GDP is highly correlated with

measures of administrative capacity and is plausibly both a cause and an effect of strong institutions. Given the empirical and conceptual relationships between poverty and state capacity, it is difficult to distinguish between the effects of each on donor behavior.[15]

Conditional on the exclusion of GDP per capita in Model 6, we observe that both differences in governance quality between countries as well as changes in governance quality within the same country over time are associated with greater aid ownership. A one-unit difference (equivalent to 2 standard deviations) between countries in governance quality is associated with a 14% increase in aid concentration. This finding indicates that donors tend to allocate more lump sum aid transfers when they perceive lower risk of aid capture or waste by extremely low-capacity governments. This is consistent with prior studies that link better governance to donors' willingness to allocate aid directly to recipient country governments as opposed to NGOs (e.g. Dietrich 2013) and echoes donors' own commitments.

In theory, the fact that donors on average allocate higher ownership aid to countries with more capable governments could indicate that donors are willing to allocate high ownership aid when they can be assured that aid money will not be diverted from development objectives. However, surprisingly, we find little evidence that recipient countries' commitment to development predicts aid concentration. In fact, liberal democratic governance correlates with less aid ownership: A one-unit difference on the Freedom House index between countries is associated with 4% less aid ownership (Model 4).[16]

Why would donors give less ownership to more democratic countries, which are presumably more committed to national development? One would expect democratic India to be better able to commit to spend lump sum aid transfers faithfully on development initiatives than autocratic Tunisia, for example, yet Tunisia receives significantly greater aid ownership than India during the period. Additionally, this finding is surprising because DAC donors have explicitly associated democratic governance with opportunities for broad-based, long run development.[17] For the

[15]Studies that argue that recipient country governance influences donors' aid allocation behavior frequently omit GDP per capita (e.g. Dietrich 2013).

[16]For robustness, we show the results using Polity score in the Appendix.

[17]See, for instance, item 3 in the OECD's 2008 *Accra Agenda for Action*.

government of the world's largest bilateral aid donor, the United States, aid-funded democracy promotion has consistently been identified as leading justification for public spending on foreign aid.[18]

One explanation for this finding might be that democracies provide donors with larger numbers of potential aid recipients. Democratic regimes are characterized by stronger civil societies and more decentralized governments than their non-democratic peers. This increases opportunities for donors to channel aid to NGOs or subnational units of government within more democratic aid-receiving countries. To enlist the expertise of these actors, donors may divide aid across a larger number of projects in more democratic states. Secondly, donors may be especially likely to use aid to pursue foreign policy and security objectives in non-democratic countries. Accordingly, they may allocate high ownership aid in pursuit of these objectives.

While this puzzling finding merits further research, it is clear that if the allocation of aid ownership was determined by the ability of aid-receiving governments' to commit to spend aid on broad-based national development, donors would allocate more, not less, aid ownership to more democratic countries. Other measures concerning countries commitment to liberal development policy also fail to predict higher levels of aid ownership. Although countries that sign PRSPs at some point during the time period tend to receive more concentrated aid than those without them (the "between" effect of PRSP in Models 4 and 5), donors do not increase aid concentration in response to the signing of a PRSP within a given country (the "within" effect of PRSP). A country's general willingness to sign the PRSP is essentially a sign of the country's broader cooperative orientation towards the donor. There is little evidence that the signing of PRSPs, however, actually constitute the first step towards greater aid ownership, as donors' have often implied (e.g., OECD 2005). Similarly, recipient country expenditure on education is not associated with greater aid ownership and increases in educational spending are in fact associated with a slight decrease in ownership over aid (Models 1-6).[19]

[18]See, for instance, U.S. National Security Strategy Doctrines released between 1994 and 2010 at: http://nssarchive.us/.

[19]This is consistent with results in, Clist, Isopi, and Morissey's (2012) study of multilateral budget aid.

Why do donors sometimes withhold aid ownership from countries that exhibit higher levels of commitment to development? Democratic countries with high commitment to development spending do not always favor the development policies that donors' do within a given country. Bolivia, for example, is a democratic state with relatively high public spending on education (with educational spending levels around 75 percent above the average aid-receiving country in 2011). Although this would presumably indicate the government's willingness to spend on development objectives, notoriously poor relations between USAID and the Bolivian government grew so contentious that the Bolivian government expelled USAID from the country in 2013. Specifically, USAID supported anti-narcotics efforts and programs to help farmers replace coca plants with other crops in Bolivia; goals that were consistent with US anti-narcotics campaigns across Latin America, but at odds with the policy preferences of Bolivian President Morales, leader of the country's coca growers' union. In this case, both donor and recipient governments may care about development, but because recipient country governments' preferences conflict with important U.S. national security objectives, USAID denied the Bolivian government opportunities to influence aid expenditure by allocating fragmented project aid.

In sum, we find that DAC donors have generally allocated low ownership aid. This suggests that donors have generally failed to adhere to their own principles about what makes aid effective for development. Moreover, if donors were using project aid as a means of bypassing corrupt governments that might divert aid towards non-developmental objectives, then variations in aid ownership would be driven primarily by recipient countries' likelihood of spending aid money on development objectives. Although we do find evidence that donors vary ownership based on government capacity, we find little evidence that they condition aid ownership primarily based on expectations about whether aid dollars will be used for development. Instead, evidence indicates that donors condition aid ownership on foreign policy alignment and security interests, to which we now turn.

3.5.4 *Interests and aid ownership*

To review, we hypothesized that donors are torn by long-run interests in promoting development within aid-receiving countries and more immediate foreign policy and security objectives that require cooperation from recipient country governments, but have little to do with development. We hypothesized that recipient country capability and demonstrated commitment to liberal development policy would therefore influence donors' behavior, but that this same behavior would be at least equally affected by donors' foreign policy and security interests. This argument is consistent with an older tradition in the aid allocation literature, but contradicts more recent studies that have excluded consideration of the role of UNGA voting alliance or military cooperation in their empirical analyses.

In accordance with our expectation, we find that UNGA voting alignment emerges as a consistent, statistically significant and substantively large predictor of aid concentration. Coefficients on this variable are positive and significant across all models: Greater UNGA voting alignment predicts greater aid ownership both between and within countries. This finding is unchanged even when we hold historical features of relationships between donors and recipients constant by using dyad fixed effects (Model 2).[20] A one standard-deviation difference between countries in the share of similar votes in the UNGA (0.20) predicts a 15% increase in aid concentration (Model 4), and a standard-deviation increase in the share of shared votes within a donor-recipient dyad predicts a 9% increase in aid concentration (Model 4). Comparing aid ownership allocated by a DAC donor country to a state that is either completely aligned or unaligned with the donor's UNGA voting patterns, we find that the aligned country receives 105% greater aid concentration than the unaligned one. These findings are robust to the substitution of a voting alignment index rather than a simple share of joint votes.[21]

The strong association between lagged UNGA voting alignment and aid ownership in the fixed effects models enables us to refine our understanding of the role that UNGA voting plays in aid

[20] Year fixed effects are included in all models to account for annual changes in the UNGA agenda.
[21] Reported in the Appendix.

allocation. Several studies have found that donors "purchase" greater alignment from developing countries by increasing the amounts of aid allocated to different countries (Dreher et al. 2008; Dreher and Strum 2012; Kuziemko and Werker 2006). Additionally, we find that donors respond to voting alignment by rewarding countries with greater ownership over foreign aid budgets both within- and across-recipients.

In a similar vein, donors also allocate more ownership to recipient country governments to whom they simultaneously transfer arms. Official arms transfers predict an 11% increase in aid concentration (Model 5). This striking finding remains strong even when we control for all historical and time-invariant features of the relationships between donors and recipients through dyadic fixed effects (Model 2).

What does this look like in practice? Consider the case of Italian aid to Yemen between 2005 and 2010. During this period, Italy allocated bilateral aid with an average aid concentration value of 28.9 across all of its aid recipients. Similarly, the average aid concentration value for Italian aid to Yemen in years without arms transfers was 26.2.[22] In 2007 and 2008, however, the Italian government undertook a foreign policy initiative intended to stem illegal immigration to Italy, which required cooperation from the Yemeni government. During these two years, Italy issued 70 arms transfers to the Yemeni Ministry of the Interior and Coast Guard and targeted the government with large, lump sum transfers to promote "the control and the management of maritime traffic across the Bab al Mandab straits" (OECD project description). As expected, Italy nearly tripled its average aid concentration to Yemen during these years (aid concentration=74.6). Armed with greater discretion over development aid budgeting, Yemen also armed its coast guard and worked to achieve strategic objectives of importance to both country governments.

Overall, these findings confirm that donors' foreign policy and security interests are the primary drivers of variation in aid ownership both across and within recipient countries. OECD donor country governments also take recipient country government capacity into account, but, despite their commitments surrounding the initiation of PRSPs, High Level Forums on Aid Effectiveness, and

[22]This includes 2005, 2006, 2009, and 2010.

in the Paris Declaration, DAC donors do not vary aid ownership on the basis of recipient country governments' commitment to liberal development policy. Moreover, although the processes by which DAC donors' allocate aid vary across countries (e.g., Lancaster 2006) and across subnational agencies (e.g., Arel-Bundock, Atkinson, and Potter, 2015), our results hold across all DAC donors between 2000 and 2010.[23] Our main findings are not driven by any single OECD donor.

3.5.5 *Shifts over time?*

Although we find no evidence that the allocation of aid ownership was influenced by recipient countries' commitments to development during the period 2000-2010, it remains possible that donors have become increasingly sensitive to factors such as democratic governance within aid-receiving countries over time. To assess whether donors have become relatively more concerned with development over time, we examine aid concentration behavior using three-year averaged models.

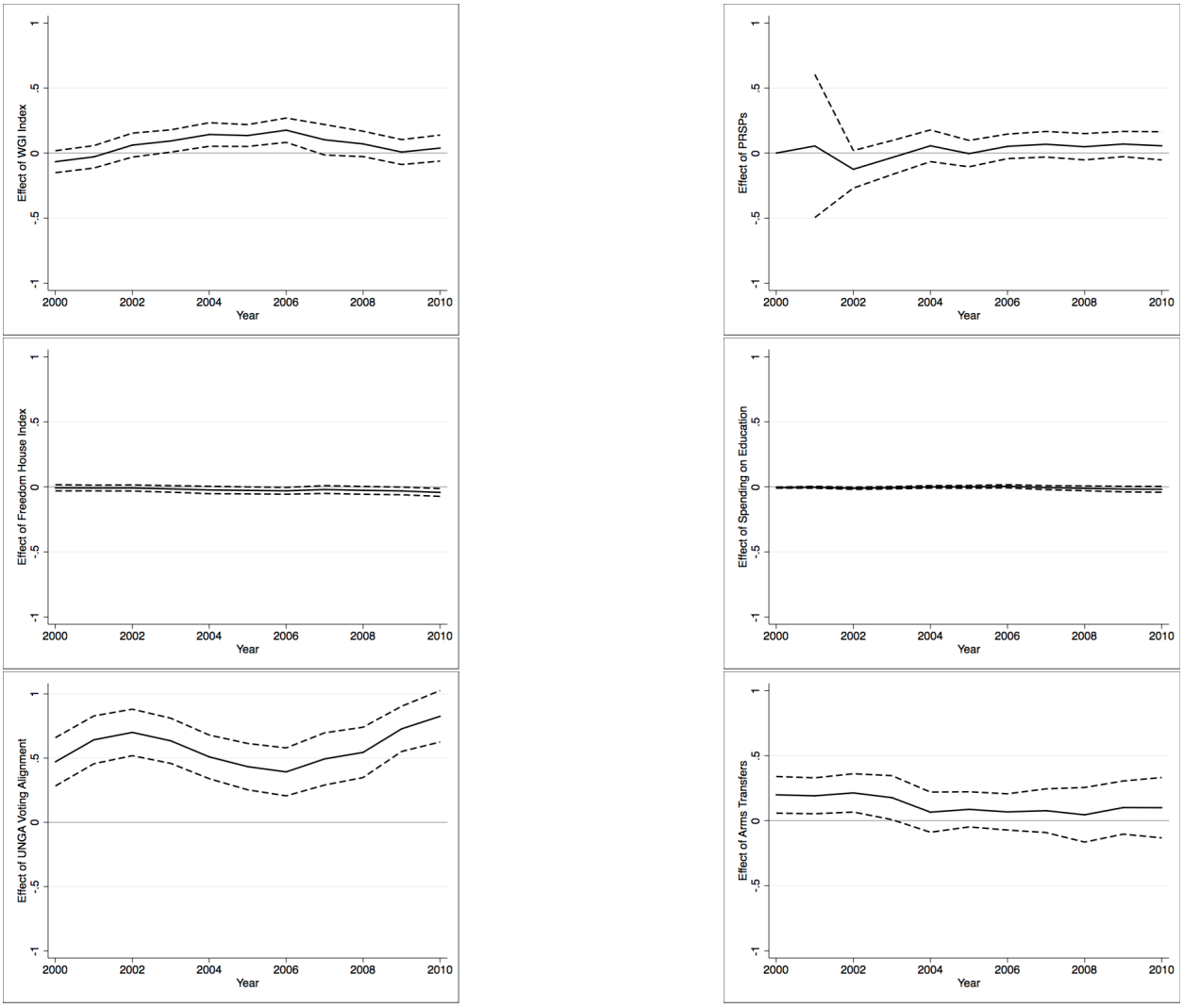
These models offer several advantages over the annual models shown above. First, they entail no risk of serial correlation. They also ensure that results are not driven by year-to-year volatility in donor behavior. Additionally, considering aid concentration averaged across several years accounts for the possibility that donors may attempt to vary ownership by dividing aid commitments over time, in addition to fragmenting aid transfers in a given year.

Figures 3a-g plot the coefficients and 90% confidence intervals from 11 separate models. Each model is the three-year averaged model of the year plotted on the x-axis and the two years prior (e.g. the coefficient plotted for 2005 is the three-year averaged model of the years 2005, 2004, and 2003). Each model allows for random effects and clustered standard errors by aid-receiving country. All models include all independent variables and controls except for logged GDP per capita.

[23] In the Appendix, we graph each coefficient while dropping one donor at a time. Although there is some variability when different donors are dropped from the analysis, none of the results are driven by single donor outliers.

Results illustrated in Figures 3a-g reinforce (rather than complicate) findings produced by the annual models. There is no evidence that donors increasingly condition aid composition on countries' adoption of PRSPs, degree of democracy, or education spending. State capacity plays a consistent but limited role in the allocation of aid ownership. Its coefficient is positive but not significant in most years. By contrast, UNGA voting alignment is not only positive and significant across all years, but the size of its effect appears to increase over time. The effect of arms transfers is consistently positive, if not always significant. This suggests that donors' international commitments to improve aid effectiveness are not reflected in their actual decisions over aid composition. Instead, donors' foreign policy and security interests and, to a lesser extent, recipient state capacity drive donors willingness to allocate aid ownership during the decade in which donors made non-binding commitments to implement the new aid paradigm.

Figure 3.3: Explaining Aid Ownership Over Time, 3-Year Average Models



Notes: Each figure plots the coefficients and 90% confidence intervals from 11 separate models. Each model is the 3-year averaged model including the year plotted on the x-axis and the 2 years prior. Each model allows for random effects and clustered standard errors by aid-receiving country. All models include all independent variables and controls except for GDP per capita. Models reported in the arms transfers figure include only those donors that record at least 1 arm transfer between 2000 and 2010.

3.5.6 *Robustness: Variation within sectors*

Donors' interest alignment with recipient country governments is a strong predictor of donors' tendency to allocate aid ownership over the entire bilateral aid flow to the recipient. However, our conclusions may be unjustified if some donors specialize in certain sectors within which aid

concentration scores commonly tend to be higher (e.g., healthcare) in certain countries.[24] As a robustness check, we therefore assess whether our findings hold for aid concentration within sectors. We examine two main sectors identified by Frot and Santiso (2010): social sector aid (including health, education, population, water supply, government, conflict prevention, gender, environment, and support to NGOs), and productive sector aid (including agriculture, forestry, fishing, industry, mining, construction, trade, and tourism).[25]

Table 3 reports results of these analyses, which are remarkably consistent even when we account for aid sector. Democratic institutions and higher educational spending do not increase the likelihood of receiving concentrated aid in either social or productive sectors. Consistent with earlier findings, countries that sign PRSPs between 2000-2010 receive more ownership on average, but signing a PRSP does not increase ownership. Moreover, higher average UNGA voting alignment over the period is associated with greater aid ownership within social sectors, and increases in UNGA voting alignment are associated with greater aid ownership within both social and productive sectors. Arms transfers also predict greater aid ownership within social and productive sectors. These results strongly support the conclusion that donors' foreign policy and security interests are the primary drivers of aid ownership.

[24]Frot and Santiso (2010) argue that a major driver of project proliferation over time has been donors' increasing allocation of aid to social sectors, which have an elective affinity for smaller project sizes than other sectors. It is worth noting that we include donor fixed effects in all models, controlling for donors' potential differential tendency to specialize in fragmented sectors.

[25]We include only those aid projects above $10,000 and donor-recipient pairs for which the total bilateral aid transfer within the sector exceeded $100,000.

Table 3.3: Explaining Aid Ownership Within Sectors, Development vs. Influence, Within-Between Random Effects Models, 2000-2010

		Model 7	Model 8	Model 9	Model 10
		Social Sectors		*Productive Sectors*	
		w/out GDP	w/ Arms	w/out GDP	w/ Arms
Between Effects					
	WGI	0.03 (0.05)	-0.02 (0.07)	-0.10 (0.04)**	-0.12 (0.05)**
	PRSP	0.16 (0.09)+	0.19 (0.10)+	0.21** (0.06)	0.16 (0.07)*
	Freedom House	-0.06 (0.02)**	-0.05 (0.02)**	-0.02 (0.01)+	-0.03 (0.01)+
	Educ. Spending (% GDP)	-0.02 (0.04)	-0.01 (0.02)	-0.00 (0.01)	-0.01 (0.01)
	UNGA	0.71 (0.28)*	0.67 (0.29)*	0.17 (0.22)	0.23 (0.22)
	Arms Transfer	0.12 (0.08)	0.18 (0.09)*	0.15 (0.08)*	0.19 (0.07)*
Within Effects					
	L.WGI-mean	0.11 (0.07)+	0.13 (0.07)*	-0.03 (0.08)	-0.05 (0.08)
	L.PRSP-mean	-0.01 (0.02)	-0.02 (0.02)	0.02 (0.04)	0.01 (0.04)
	L.Freedom House-mean	0.01 (0.01)	0.01 (0.01)	-0.01 (0.01)	-0.01 (0.01)
	Educ. Spending (% GDP)-mean	-0.01 (0.00)**	-0.01 (0.00)*	-0.02 (0.00)**	-0.01 (0.01)
	L.UNGA-mean	0.27 (0.14)+	0.44 (0.17)**	0.73 (0.17)**	0.92 (0.19)**
	Arms Transfer-mean	0.05 (0.05)	0.05 (0.05)	-0.09 (0.04)*	-0.09 (0.04)*
	Donor Fixed Effects	Y	Y	Y	Y
	Recipient Fixed Effects	Y	Y	Y	Y
	Year Fixed Effects	Y	Y	Y	Y
	Observations	9,793	8,003	6,320	5,223

*Notes: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Standard errors in parentheses and are clustered by aid-receiving country. For all models, logged aid concentration score within sectors is the dependent variable. Donor-recipient dyads with no positive aid flows or aid flows within sector of less than $100,000 are excluded. All models include donor and year fixed effects and random effects by aid-receiving country. Model 8 and 10 include only those donors that record at least 1 arms transfer between 2000 and 2010. Within and between effects are included, but not reported, for all control variables.*

3.5.7 *Robustness: General Budget Support*

Although aid concentration offers a more continuous measure of aid ownership, we assess the robustness of our findings by examining donors' tendency to allocate General Budget Support (GBS), which we report in our Appendix. Given the censoring in the data, we use a double-hurdle model. The first stage is a logit model with a dependent variable equal to one if a donor

allocates GBS to a given recipient and zero if the donor allocates aid other than GBS. The second stage estimates the amount of GBS as a (logged) share of the donor's bilateral aid to the recipient, conditional on receiving GBS.

Results reveal a similar picture about the allocation of aid ownership. While donors are more likely to allocate GBS to countries that have signed PRSPs, they are no more likely to allocate a greater share of bilateral aid as GBS. More democratic recipients and recipients with higher levels of education spending are not more likely to receive GBS or higher shares of GBS, conditional upon its allocation. Surprisingly, countries with greater capacity are generally less likely to receive GBS, though improvements in capacity are associated with greater likelihood of receiving GBS.

Collectively, these results suggest a limited role for commitment to development as a criterion for allocating GBS. Instead, conditional on receiving GBS, UNGA voting alignment and arms transfers from donors predict significantly greater shares of GBS: One standard deviation difference in UNGA voting alignment (0.20) is associated with 51% more budget aid, and countries receiving arms transfers from particular donors receive 272% more GBS.

3.6 Conclusions

In this paper, we have argued that donors typically allocate aid ownership in pursuit of diverse strategic objectives, including but not limited to development. We find strong support for this argument. Donors' foreign policy and security interests are consistently stronger predictors of the allocation of aid ownership than recipients' commitment to liberal development policy. Recipient country capacity also influences the allocation of aid ownership, but the relationship between governance and aid ownership is not always robust. Despite donors' public commitments, we find no evidence that the influence of donors' strategic incentives has decreased over time. These results are robust to substitution of alternative dependent variables and a number of modeling specifications.

Additionally, this paper has illuminated the various policy levers that donors use to vary aid ownership. To date, studies have typically focused either on the allocation of budget aid or the use of recipient country systems. We advance the literature by identifying aid concentration as a more reliable measure of aid ownership.

Finally, results reported in this paper are relevant for debate over aid reform. In seeking to explain project proliferation, policymakers have emphasized technocratic obstacles to the allocation of greater aid ownership, such as redundancy of efforts across donors operating within an aid-receiving country. Accordingly, reformers have invested heavily in promoting intra-donor coordination and an improved "division of labor" among donors. Yet, our results call this emphasis into question: Although we find that higher numbers of donors in a country predict less concentrated aid, the substantive influence of multiple donors is small. It would take the removal of 4 OECD donors to equal the substantive effect of a change in governance quality, and far more to rival the change induced by UNGA voting alignment. Emphasis on donor coordination may obscure more substantively significant, political determinants of aid ownership.

Ultimately, our findings support Barder's (2009) critique of the "planning" approach to reform. Whereas many reformers believe that changes in bureaucratic rule-making or intra-agency cooperation will reduce aid fragmentation, we argue that a larger shift in the balance of power over aid expenditure is required before bilateral donors are likely to maximize the potential impact – and minimize the potential harm – of development aid. Understanding the strategic dimension of aid selectivity and composition is not a silver bullet for reform, but it is a prerequisite for serious change.

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3.8 Appendix

3.8.1 *Appendix Tables and Figures*

Table 3.4: List of DAC donors ranked by amount of bilateral aid (2000-2010)

	Total Bilateral Aid (billions 2005 USD)	Average Aid Concentration
United States	$199.9	23.0
Japan	$132.5	33.2
Germany	$62.1	27.0
France	$51.8	30.1
United Kingdom	$46.2	38.1
The Netherlands	$25.4	35.8
Spain	$20.9	22.8
Australia	$16.7	32.3
Canada	$16.6	32.1
Norway	$15.4	22.9
Sweden	$12.8	31.1
Denmark	$12.5	39.0
Italy	$9.2	31.9
Belgium	$8.9	26.7
Switzerland	$8.3	24.9
Finland	$5.0	37.5
Ireland	$4.5	20.0
Austria	$3.5	39.3
Portugal	$3.4	47.9
Luxembourg	$1.9	31.3
Greece	$1.5	31.6
New Zealand	$1.1	27.0

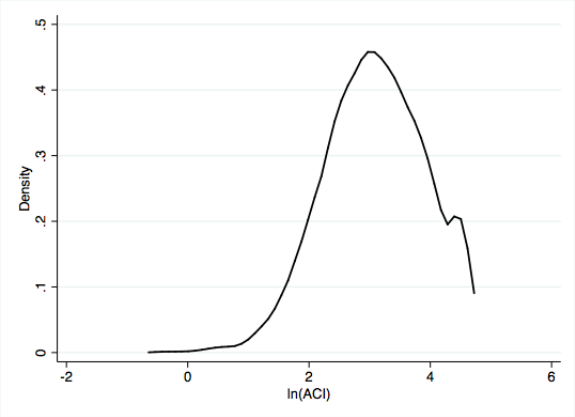
Notes: Bilateral development aid totals and aid concentration values exclude debt forgiveness and donor expenses for refugees living in donor countries. We exclude Czech Republic, Iceland, Poland, Slovak Republic, Slovenia, and South Korea as these countries all became DAC members in 2010 or later.

Table 3.5: List of DAC donors that record arms transfers, 2000-2010

	Number of arms transfers within sample
United States	199
France	116
Germany	89
Italy	57
United Kingdom	42
Spain	30
Canada	29
The Netherlands	22
Sweden	16
Switzerland	11
Belgium	10
Finland	5
Australia	4
Denmark	3
Norway	2
Greece	1

Notes: Arms transfers as recorded in SIPRI database. Summary statistics include only those arms transfers in the sample.

Figure 3.4: Distribution of Aid Concentration, 2000-2010



Notes: Kernel density plot of (logged) aid concentration index.

Table 3.6: Summary of Variables, Including Variance Between Donor-Recipient Dyads

	Summary statistics			Variance between dyads	
	Mean	Min	Max	Total Variance	VPC
Aid Concentration (log)	3.08	-0.53	4.61	0.70	0.51
WGI	-0.47	-1.67	1.51	0.32	0.95
PRSP	0.34	0	1	0.23	0.56
Freedom House	4.84	1	7	2.69	0.83
Edu. Spending (% GDP)	4.34	0.58	31.3	6.33	0.75
UNGA Voting Alignment	0.73	0	1	0.04	0.90
Arms Transfer	0.06	0	1	0.06	0.50
Number of Donors	17.32	1	23	16.4	0.79
Observations			10,240		

Notes: Table 6 reports summary statistics for all variables included in the sample. The variance statistic reports total variance for each variable, and the Variance Partition Coefficient (VPC) reports the proportion of the total variance that occurs between dyads.

Table 3.7: Differing Project Threshold Cutoffs, 2000-2010

		Model 1 *No project threshold*	Model 2 *$50,000 project threshold*
Between Effects			
	WGI	0.13 (0.05)*	0.14 (0.05)**
	PRSP	0.13 (0.09)	0.10 (0.08)
	Freedom House	-0.05 (0.02)**	-0.04 (0.02)*
	Educ. Spending (% GDP)	-0.01 (0.01)	-0.01 (0.01)
	UNGA	0.75 (0.29)**	0.73 (0.31)*
	Arms Transfer	0.15 (0.09)	0.13 (0.097)*
Within Effects			
	L.WGI-mean	0.12 (0.07)+	0.12 (0.07)+
	L.PRSP-mean	-0.02 (0.03)	-0.03 (0.03)
	L.Freedom House-mean	0.01 (0.01)	0.00 (0.01)
	Educ. Spending (% GDP)-mean	-0.01 (0.01)*	-0.01 (0.01)+
	L.UNGA-mean	0.46 (0.13)**	0.43 (0.13)**
	Arms Transfer-mean	0.11 (0.05)*	0.11 (0.05)*
	Donor Fixed Effects	Y	Y
	Recipient Fixed Effects	Y	Y
	Year Fixed Effects	Y	Y
	Observations	10,240	10,239

*Notes: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Standard errors in parentheses. For all models, logged aid concentration score is the dependent variable. Model 1 calculates aid concentrated score without using any project size cutoffs. Model 2 calculates aid concentration score including only those projects of $50,000 or above. Donor-recipient dyads that include no positive aid flows or aid flows of less than $1M are excluded in all models. All models include donor and year fixed effects and random effects by aid-receiving country. Within and between effects are included, but not reported, for all control variables as well, including: trade intensity (log), UNSC membership, former colony, civil conflict, total DAC aid (log), aid-receiving country's rank in donor's aid portfolio, and population (log). Standard errors for all models are clustered by aid-receiving country.*

Figure 3.5: Differing Measures for UNGA Voting Alignment and Democracy, 2000-2010

		Model 3 *Voting Index*	Model 4 *Polity*
Between Effects			
	WGI	0.01 (0.07)	0.08 (0.07)
	PRSP	0.19 (0.10)*	0.17 (0.09)
	Freedom House	-0.03 (0.02)+	
	Polity		-0.01 (0.00)**
	Educ. Spending (% GDP)	-0.00 (0.01)	-0.00 (0.01)
	UNGA Voting Index	0.31 (0.15)*	
	UNGA Voting Alignment		0.79 (0.28)**
	Arms Transfer	0.15 (0.09)	0.16 (0.09)+
Within Effects			
	L.WGI-mean	0.11 (0.07)**	0.12 (0.08)
	L.PRSP-mean	-0.03 (0.03)	-0.03 (0.03)
	L.Freedom House-mean	0.01 (0.01)	
	L.Polity-mean		-0.00 (0.01)
	Educ. Spending (% GDP)-mean	-0.01 (0.01)*	-0.01 (0.01)*
	L.UNGA Voting Index-mean	0.24 (0.07)**	
	L.UNGA Voting Alignment		0.50 (0.14)**
	Arms Transfer-mean	0.12 (0.05)*	0.12 (0.05)**
	Donor Fixed Effects	Y	Y
	Recipient Fixed Effects	Y	Y
	Year Fixed Effects	Y	Y
	Observations	9,989	9,989

*Notes: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Standard errors in parentheses. For all models, logged aid concentration score is the dependent variable. Donor-recipient dyads that include no positive aid flows or aid flows of less than $1M are excluded in all models. All models include donor and year fixed effects and random effects by aid-receiving country. Within and between effects are included, but not reported, for all control variables as well, including: trade intensity (log), UNSC membership, former colony, civil conflict, total DAC aid (log), aid-receiving country's rank in donor's aid portfolio, and population (log). Standard errors for all models are clustered by aid-receiving country.*

Table 3.8: Including Total Aid as Control, Fixed Effects and Random Effects Models, 2000-2010

	Model 5 FE	Model 6 FE	Model 7 RE
L.WGI	0.086 (0.054)	0.076 (0.063)	0.070 (0.042)+
L.PRSP	-0.000 (0.030)	-0.031 (0.032)	0.013 (0.027)
L.Freedom House	-0.006 (0.012)	0.002 (0.012)	-0.011 (0.010)
L.Educ. Spending (% GDP)	-0.018 (0.005)**	-0.019 (0.005)**	-0.011 (0.003)**
L.UNGA	0.512 (0.160)**	0.443 (0.132)**	0.440 (0.140)**
Arms Transfer	0.091 (0.052)+	0.111 (0.046)*	0.100 (0.052)*
UNSC	0.020 (0.037)	0.021 (0.039)	0.017 (0.037)
Former Colony	-0.403 (0.072)**		-0.410 (0.070)**
Civil Conflict	0.007 (0.039)	-0.015 (0.039)	0.007 (0.034)
Total DAC Aid (log)	0.089 (0.020)**	0.030 (0.020)	0.063 (0.017)**
Recipient rank in donor portfolio	-0.004 (0.001)**	0.001 (0.001)	-0.004 (0.001)**
L.Population (log)	0.241 (0.315)	0.133 (0.360)	-0.042 (0.023)**
L.GDP per capita (log)	0.194 (0.113)+	0.217 (0.121)+	0.111 (0.031)**
L.Number of donors	0.001 (0.005)	-0.004 (0.006)	-0.012 (0.005)**
L.Trade Intensity	-0.058 (0.013)**	0.011 (0.015)	-0.056 (0.012)**
Bilateral aid (log)	-0.010 (0.019)	0.152 (0.024)	-0.011 (0.019)
Donor Fixed Effects	Y	N	Y
Recipient Fixed Effects	Y	N	N
Dyad Fixed Effects	N	Y	N
Year Fixed Effects	Y	Y	Y
Observations	10,240	10,240	10,240

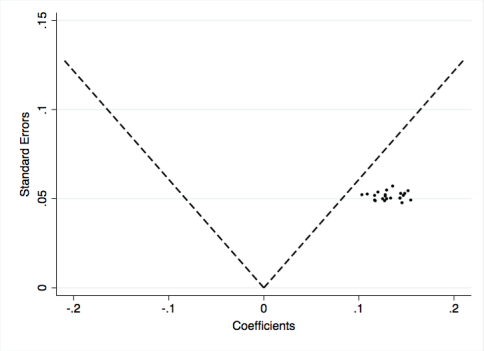
*Notes: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Standard errors in parentheses. For all models, logged aid concentration score is the dependent variable. Donor-recipient dyads that include no positive aid flows or aid flows of less than $1 million are excluded in all models. Model 5 includes donor, recipient, and year fixed effects. Model 6 includes dyad and year fixed effects. Model 7 includes random effects by dyad and donor and year fixed effects. Standard errors for all models are clustered by aid-receiving country.*

Table 3.9: Including Total Aid as Control, Within-Between Random Effects Models, 2000-2010

		Model 8 *Full Sample*	Model 9 *w/ Arms*	Model 10 *w/out GDP*
Between Effects				
	WGI	0.002 (0.068)	0.047 (0.065)	0.002 (0.068)
	PRSP	0.2342 (0.094)*	0.185 (0.093)*	0.233 (0.094)*
	Freedom House	-0.039 (0.017)*	-0.039 (0.018)*	-0.039 (0.017)*
	Educ. Spending (% GDP)	-0.008 (0.012)	0.001 (0.014)	-0.008 (0.012)
	UNGA	0.573 (0.343)+	0.620 (0.348)+	0.573 (0.343)+
	Arms Transfer	0.125 (0.091)	0.228 (0.095)*	0.125 (0.091)
Within Effects				
	L.WGI-mean	0.086 (0.065)	0.123 (0.069)+	0.086 (0.065)
	L.PRSP-mean	-0.026 (0.033)	-0.0214 (0.037)	-0.026 (0.033)
	L.Freedom House-mean	0.002 (0.012)	-0.002 (0.013)	0.002 (0.012)
	Educ. Spending (% GDP)-mean	-0.015 (0.006)*	-0.016 (0.006)*	-0.015 (0.006)*
	L.UNGA-mean	0.478 (0.138)**	0.637 (0.152)**	0.478 (0.138)**
	Arms Transfer-mean	0.104 (0.044)*	0.106 (0.045)*	0.104 (0.044)*
	Donor Fixed Effects	Y	Y	Y
	Recipient Fixed Effects	Y	Y	Y
	Year Fixed Effects	Y	Y	Y
	Observations	10,240	8,372	10,240

*Notes: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Standard errors in parentheses. For all models, logged aid concentration score is the dependent variable. Donor-recipient dyads that include no positive aid flows or aid flows of less than $1 million are excluded in all models. Model 8 includes donor and year fixed effects and random effects by aid-receiving country. Model 9 includes only those donors that record at least 1 arms transfer between 2000 and 2010 (listed in Supplementary Materials). Within and between effects are included, but not reported, for all control variables as well, including: trade intensity (log), UNSC membership, former colony, civil conflict, total DAC aid (log), aid-receiving country's rank in donor's aid portfolio, population (log), GDP per capita (log), and bilateral aid (log). Model 10 excludes GDP per capita as a control. Standard errors for all models are clustered by aid-receiving country.*

Figure 3.6: Coefficients and Standard Errors for Between- and Within-Effects of WGI, Dropping One Donor at a Time



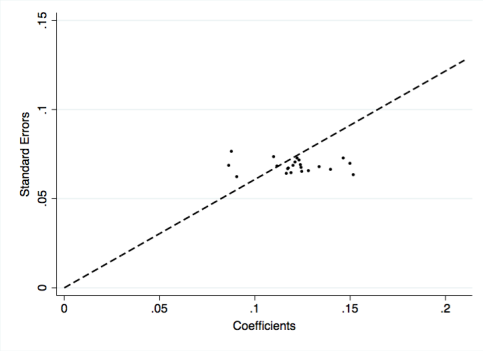
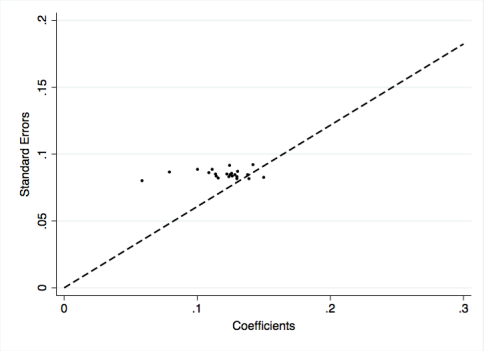


Figure 3.7: Coefficients and Standard Errors for Between- and Within-Effects of PRSP, Dropping One Donor at a Time



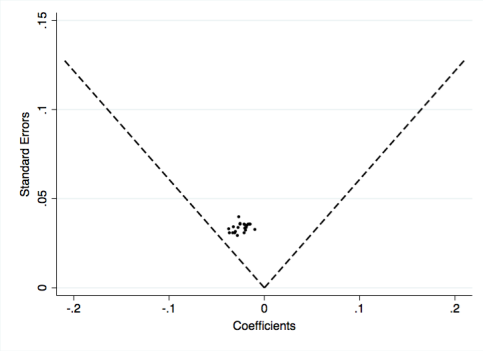
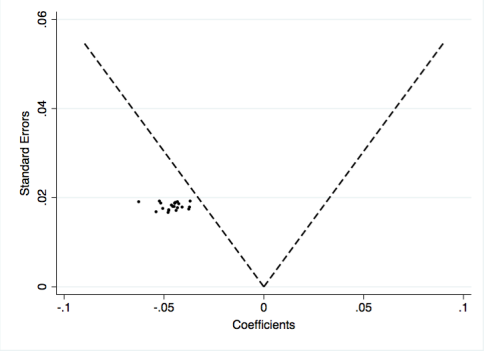


Figure 3.8: Coefficients and Standard Errors for Between- and Within-Effects of Freedom House Index, Dropping One Donor at a Time



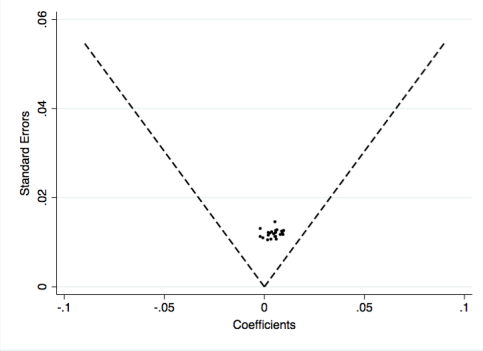
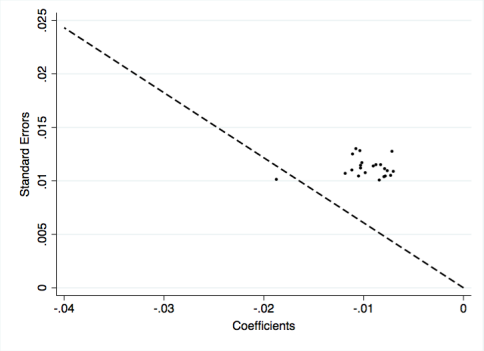


Figure 3.9: Coefficients and Standard Errors for Between- and Within-Effects of Educational Spending, Dropping One Donor at a Time



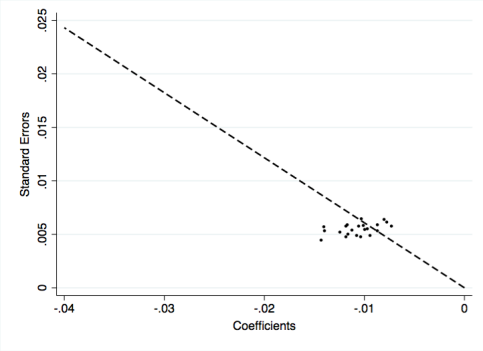
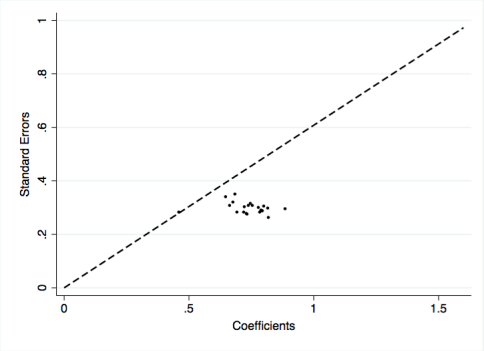


Figure 3.10: Coefficients and Standard Errors for Between- and Within-Effects of UNGA Voting Alignment, Dropping One Donor at a Time



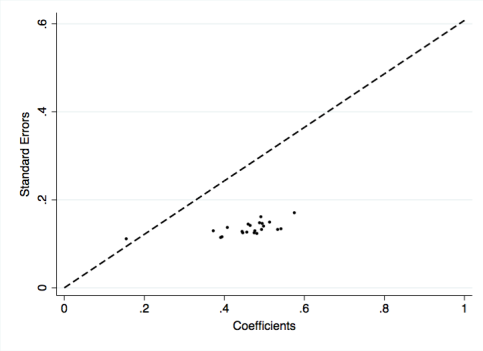
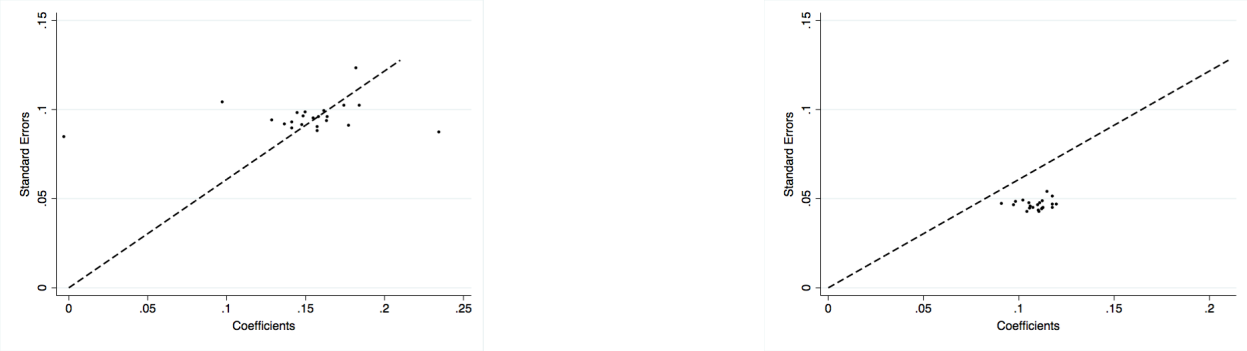


Figure 3.11: Coefficients and Standard Errors for Between- and Within-Effects of Arms Transfers, Dropping One Donor at a Time



Notes: Each figure plots the coefficients and standard errors from 22 separate within-between random effects models. Dashed lines indicate statistical significance at the 90% level. Each model drops all observations from a single donor. For all models, logged aid concentration score is the dependent variable. Donor-recipient dyads that include no positive aid flows or aid flows of less than $1 million are excluded in all models. All models include donor and year fixed effects and random effects by aid-receiving country. Within and between effects are included, but not reported, for all control variables as well, including: trade intensity (log), UNSC membership, former colony, civil conflict, total DAC aid (log), aid-receiving country's rank in donor's aid portfolio, and population (log). GDP per capita (log) is excluded. Standard errors for all models are clustered by aid-receiving country.

3.8.2 *Supplementary Measurement Discussion: Government as Implementing Agency as a Measure of Aid Ownership?*

Donors' tendency to use recipient country government systems for managing aid is an important component of the overall agenda for aid reform, articulated most clearly in the Paris Declaration. Delivering aid using donors' (rather than recipients') reporting, procurement, and monitoring procedures can undermine long-run institutional development and creates further separation between project implementers and intended aid beneficiaries, reducing accountability in aid-giving and foreclosing opportunities for local knowledge to guide development policies. However, when donors are concerned about the short-run success of a specific project, there are key short-run benefits to delivering aid through their own systems. They can better ensure that aid dollars are not diverted from specific project objectives when they retain control. Noting this tension, the World Bank's *Assessing Aid* report argues that: "At times, donors have hindered the creation of effective public sectors because they saw end runs around local institutions as the easiest way to achieve project success" (World Bank, 1998: 84).

In the Paris Declaration, donors commit to delivering aid in ways that are more consistent with long-run institutional development, including channeling aid through recipient governments' public sectors. As we note in the paper, however, this commitment came with the recognition that channeling aid through public sectors with poor governance and records of corruption would result in aid diversions and that donors could reasonably maintain control of aid in these instances. In line with this logic, Knack (2013) and Dietrich (2013) find that donors are more likely to engage recipient systems when the quality of governance is high.

However, examining donors' tendency to channel aid through recipient systems in isolation can yield misleading conclusions about overall levels of 'aid ownership.' Many of the aid-receiving countries most active in the struggle for aid reforms to promote greater ownership already receive large sums of aid through their own systems (e.g. Tanzania). In these cases, donors do channel aid through recipient systems, but fragment aid to such an extent that they still maintain control over individual projects and end up over-burdening recipient governments. This means of delivering

aid can be equally detrimental to long-run institutional development. Thus, as we note in the main paper, we favor considering aid concentration—alternatively, the extent to which aid is fragmented across projects—as an indicator that more closely align with the notion of aid ownership (one that does not undermine recipients' long-run institutional development) central to the aid reform agenda since 2000.

However, given the use of this indicator in the literature, it's worthwhile to examine whether recipient countries' commitment to liberal development, in addition to quality of governance, explains donors' tendency to channel aid through public sectors. Alternatively, donors' foreign policy and security interests could shape their tendency to use recipient systems, as we find using our preferred measures. Before showing these results, it is worth noting some serious constraints on donors' reporting of implementing agency. In 2004, the OECD began to collect voluntary information from donors regarding "aid channel." Donors report whether aid for a particular aid project is directed through one of six different types of implementing agents: to donor government agencies, to recipient government agencies, to non-governmental organizations, to public-private partnerships, to multilateral institutions, and to other institutions (including, for example, universities and research centers). However, reporting on aid channel is voluntary, and there is significant missing data. 21% of bilateral aid projects by DAC donors, accounting for 28% of bilateral aid flows, between 2005 and 2010 contain missing data on aid channel.

Even data that is not technically missing is frequently misleading. For example, the United States during much of the period only designates whether a project was implemented by a public sector agency or not but does not distinguish between agencies within donor and recipient countries. So, a project implemented by USAID and a project implemented by the recipient country government could receive the same implementing agency code ("public sector"). These two scenarios obviously represent extreme differences in the level of ownership for recipient country governments.

To mitigate missing data problems, we examined aid project descriptions for information on implementing agent that may not have been reported in the implementing agent field. We reviewed

147,261 separate projects and were able to determine whether or not the project utilized recipient country systems for 68,545 of the projects. However, this review also caused us to reclassify over 75,000 aid projects as missing data in cases where donors reported using public agencies as implementing agents but failed to distinguish between donor- and recipient-country agencies. This results in missing data on aid channel for 36% of all aid projects, representing 44% of bilateral aid flows for the 2005-2010 period.[26] Thus, in addition to the fact that use of recipients' public sector does not guarantee aid ownership, there are empirical limitations with using the data. Nonetheless, we find the exercise valuable so that we can more fully engage the existing literature.

Due to the missing data and the more limited time frame of the data, we employ a simple modeling strategy. Units of observation are donor-recipient dyad over the entire period between 2005 and 2010. For the dependent variable, we calculate aid traveling through recipient systems over the entire time period. Independent variables are averaged over the period. We favor the averaged model over annual models in this case because the time period is relatively short (i.e. there are not many changes in the values of the independent variables) and because of the problems caused by the missing data. Missing data would cause certain donor-recipient dyads to fall in and out of the analysis during different years, potentially resulting in sample selection problems. Instead, we simply include the donor-recipient dyads for which at least 75% of the data on aid channel is reported over the entire time period. Dyads with significant data are therefore excluded entirely, rather than jumping in and out of the analysis due to selective reporting in certain years. Of course, this still results in a selective sample of countries on which donors choose to report aid channel, but at least the rule for inclusion in the sample is consistent and does not result in a volatile sample.

Given the censoring in the data, we use a double-hurdle model. The first stage is a logit model with a dependent variable equal to one if a donor allocates any aid through the public sector to a given recipient during the period and zero if the donor allocates aid but bypasses recipients' public

[26]We only count as missing those projects where we could not positively identify whether or not the recipient country government was the implementing agency. If we could determine that the recipient country government was not the implementing agency (e.g. because the donor stated that it used an NGO), we counted this as aid bypassing the government even if we could not positively identify which non-governmental agency implemented the project.

sectors. The second stage estimates the amount of aid channeled through recipient systems as a (logged) share of the donor's bilateral aid to the recipient, conditional on receiving any aid through the public sector.

Results are broadly consistent with findings in the main paper. In particular, greater UNGA voting alignment between donors and recipients is associated with higher shares of aid traveling through recipient governments.

Table 3.10: Explaining Aid Through Public Sectors in Aid-Receiving Countries, Averaged Models, 2005-2010

	Model 1 *Get Gov't*	Model 2 *Share Gov't*
WGI	0.367 (0.197)+	1.018 (0.281)**
PRSP	0.508 (0.194)**	1.146 (0.365)**
Freedom House	-0.012 (0.052)	0.006 (0.075)
Educ. Spending (% GDP)	0.076 (0.047)	0.069 (0.083)
UNGA	0.238 (0.571)	4.160 (1.232)**
Arms Transfer	-0.691 (0.473)	0.046 (0.558)
UNSC	0.234 (0.202)	-0.667 (0.280)*
Former Colony	0.492 (0.943)	-0.100 (0.376)
Civil Conflict	0.070 (0.203)	-0.183 (0.281)
Total DAC Aid (log)	0.143 (0.137)	0.372 (0.120)+
Recipient rank in donor portfolio	0.033 (0.004)**	0.007 (0.004)
Population (log)	-0.299 (0.098)**	0.225 (0.151)
Trade Intensity (log)	0.139 (0.036)**	0.009 (0.069)
Observations	993	314

*Notes: ** p-value < 0.01, * p-value <0.05, + p-value < 0.10. Standard errors in parentheses.*